

MIND

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OF

PSYCHOLOGY AND PHILOSOPHY



I.—THE FOUNDATIONS OF PROBABILITY.

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EVERYONE has some idea of the meanings of the terms 'probable' and 'improbable'; yet no attempt to make precise the exact nature of these concepts can be said to have been successful. Certainly none has been free from criticism.

The best known attempts to establish the theory of Probability on a secure foundation are the various forms of the 'Frequency Theory.' The general feature of all such attempts is that they make the probability of an event dependent on the frequency with which the event has happened in the past. Thus, omitting refinements of the theory, the reason given for believing that the probability of 'heads,' when a coin is spun, is $\frac{1}{2}$, is that heads has appeared in half the number of previous throws.

Nearly every writer on Probability has made extensive use of the Frequency Theory in some form or another; but nearly every writer has also made wide use of a psychological theory of Probability, which makes probability dependent on some such conception as 'a degree of knowledge'. Such theories of Probability make use of a general principle, known alternatively as "the Principle of Sufficient Reason," "The Principle of Non-Sufficient Reason," and "The Principle of Indifference." The last name is due to Mr. J. M. Keynes, and will be used here.

The Principle of Indifference also makes the probability of a spun coin throwing heads to be $\frac{1}{2}$, but it has quite different reasons from the Frequency Theory for doing so. Again omitting refinements of the theory, we may say that the Principle of Indifference makes the probability of heads $\frac{1}{2}$,

because we have no evidence which leads us to expect heads rather than tails, or vice versa.

It is only quite recently that any attempt has been made to examine comprehensively the relation between these theories. Such an examination is obviously necessary, for it is not certain that the two principles will always lead to consistent results, nor that they are applicable to the same range of subjects.

In 1921, Mr. J. M. Keynes published a critical comparison of the theories;¹ and he arrived at the conclusion that the Frequency Theory was entirely subordinate to the psychological or logical theory. He stated that his criticisms had been much handicapped by the absence of a recent logical exposition of the Frequency Theory, and therefore he suggested possible answers to some of his own criticisms; but, finally, he came to the following conclusions:—

(1) The Frequency Theory can be validly applied, but only in certain branches of the subject, and only under certain conditions.

(2) What these restrictions are, can be determined by developing the Theory of Probability from quite different foundations.

(3) In these valid applications, the Frequency Theory becomes merely a part of the development of the Theory of Probability from the other foundations.

(4) This other treatment of Probability can be applied to branches of the subject, to which the Frequency Theory cannot be applied.

In addition to these criticisms, there remain many, which Mr. Keynes himself has answered. If anyone should now bring forward a Frequency Theory, while not accepting these answers, he should provide alternatives.

At about the same time as Mr. Keynes' Treatise, there appeared another account of Probability, by Dr. Norman Campbell.² This must be classified as a Frequency Theory, and it is undoubtedly a much more complete and logical exposition of a frequency theory than any previous one. Yet a large number of Mr. Keynes' criticisms remain unanswered, so that there will be many who feel that a frequency theory is still rather waste of time.

I believe not only that Mr. Keynes' criticisms can be successfully met, but also that Mr. Keynes' system can be

¹ *A Treatise on Probability*, by J. M. Keynes, 1921.

² "Physics: The Elements," Dr. Norman Campbell, 1920.

"The Measurement of Chance," Dr. Norman Campbell, *Phil. Mag.*, July, 1922.

shown to be itself so open to criticism, that it must be discarded in favour of a Frequency Theory.

Let us take first the objections to Mr. Keynes' system. For this purpose we may take the following as a rough summary of his leading ideas:—

The usual form of statement—"Such and such a proposition is probable"—is elliptical; it really stands for "such and such a proposition is probable in view of such and such evidence". Therefore we may say that the proposition a is probable with respect to evidence h , but we cannot merely say " a is probable," any more than we can say " x is greater than".

Probability itself is a primitive unanalysable notion. We know that a is probable on evidence h , only because we are directly aware of some special kind of logical relation between a and h , by means of which we know that a follows from h , but not with certainty.

This kind of logical relationship between propositions is capable of degree, and we have, in some cases, a direct perception of the magnitude of this degree. Suppose, in the case of a and h , that we know the degree of the relation to be a ; then we write $\frac{a}{h} = a$, and read it, "The probability of a on evidence h (or on hypothesis h) is a ."

According to this view, therefore, any properties possessed by degrees of probability are consequences of the properties possessed by our perceptions of the degrees of this special kind of logical relation between propositions.

The most important of these properties (which we are supposed to recognise intuitively as being possessed by these perceptions) is that enunciated in the Principle of Indifference:—

If there are a finite number of alternatives, a, b, c, \dots etc., and if the evidence, h , is not favourably relevant to some only of the alternatives, then the probabilities of all the alternatives will be equal, i.e., $\frac{a}{h} = \frac{b}{h} = \frac{c}{h} = \text{etc.}$

If there are only n such alternatives, and they are known by the P. of I. to be all equal, we can say that the probability of each of the alternatives is $\frac{1}{n}$. Thus it is by the P. of I. that we arrive at probabilities which have numerical values.

It will be noticed that, before the P. of I. can be applied, the evidence, h , has to be judged to be not specially favourably relevant to some of the alternatives. It follows that judgments of irrelevance are the fundamental judgments in

Probability. It is therefore of importance to discuss these judgments, and to consider how they are to be formed. Mr. Keynes does not specifically do this, but he does give a definition of irrelevance:—

Evidence h_1 is irrelevant to the probability of a on evidence h , if the addition of h_1 to h does not alter the probability, *i.e.*, if $\frac{a}{hh_1} = \frac{a}{h}$.

This definition, however, does not give the slightest assistance in forming a judgment of irrelevance. Suppose that there are only two alternatives a and b , and we wish to find their probability on evidence h and h_1 . Suppose, also, that we know that h is irrelevant. Then, if we can make the judgment that h_1 is also irrelevant, we can apply the P. of I. to the two alternatives and we shall be able to say that $\frac{a}{hh_1} = \frac{b}{hh_1} = \frac{1}{2}$. How are we to make this judgment of irrelevance? From the definition, we shall be able to make it if we are able to say that $\frac{a}{hh_1} = \frac{a}{h}$. Now, since h is irrelevant, we know $\frac{a}{h}$ and we can say that it is equal to $\frac{1}{2}$. Therefore, if we can say that $\frac{a}{hh_1} = \frac{1}{2}$, we shall know that $\frac{a}{hh_1} = \frac{a}{h}$ and therefore that h_1 is irrelevant. But this is precisely what we cannot do, for it is in order to be able to say that $\frac{a}{hh_1} = \frac{1}{2}$ that we require to know that h_1 is irrelevant.

It may be replied that the definition of irrelevance is not intended to be used as a criterion of irrelevance: in that case, we have still to find a criterion of irrelevance, and have still to discuss how to make judgments of irrelevance. As an example, let us take Mr. Keynes' instance of the drawing of black and white balls from an urn. If it is known that the black balls are of iron, and the white ones of tin, and that the balls are drawn out with a magnet, this is relevant evidence affecting the probabilities. But, surely, it is so because we have had experience that the conditions will favour the frequency of black balls, and because we know that magnets frequently attract iron.

It seems to me that, through considerations such as judgments of relevance, all psychological theories of probability will be found to be ultimately based on experience of frequencies.

However this may be, there is a much more serious criticism to be made of Mr. Keynes' development of the subject. It is that the laws and theorems of probability, which he

appears to deduce from the very nature of the idea of probability, cannot really be obtained from his starting-point. His apparent success in so doing depends on what must be a rather obscure fallacy, since, as far as I know, it has not previously been pointed out.

If probability is some such conception as a degree of knowledge, or a degree of logical relation between propositions, it is not immediately obvious what can be meant by adding, or multiplying, together the probabilities of two different propositions. If a meaning is to be given to these processes, it must be given by definition. This is what Mr. Keynes does. For example, he defines the product of two probabilities of

the forms $\frac{a}{bh}$ and $\frac{b}{h}$ as $\frac{ab}{h}$. If we take h as the general body

of our knowledge, this may be expressed, rather loosely, in ordinary terms as follows: the product of the-probability-of- b with the-probability-of- a -if- b -is-true is defined to be the-probability-of- a -and- b -together. This definition agrees with ordinary usage, for it is generally held that the probability that two events will occur together is the product of the probability that the first will occur and the probability that the second will occur when the first has occurred.

Mr. Keynes goes on to state axioms of probability, such as his axiom (vi.) :—

If P , R , S are probabilities, and if the probabilities $R \pm S$, PR , PS exist as probabilities, then $P(R \pm S) = PR \pm PS$.

Now, in what sense is this an axiom? The modern sense of the word axiom is a statement about otherwise undefined entities. But the probabilities P , R , S being given, all the probabilities mentioned in the 'axiom' are defined quantities, and their nature is definitely determinate, since they are the probabilities of definite statements on definite evidence. Any relation, stated between definite entities of a special kind, is a law, not an axiom.

The older meaning of the term 'axiom' was a law which is obviously true. Even in this sense, it cannot be claimed that axiom (vi.) is really an axiom. Given Mr. Keynes' definition, or any other 'psychological' definition, of probability, I do not think that anyone can conscientiously claim that axiom (vi.) is an obviously true law for probabilities.

In any case, axiom (vi.) is really a law of probability, and this law has not been proved by Mr. Keynes; nor do I think that it can be proved, if probability is established on a psychological basis. Whatever merits the psychological view may have, it does not have that of establishing the laws of probability.

These axioms, which I believe that Mr. Keynes has really assumed as laws, are the axioms which express the commutative and associative laws of addition and multiplication. If they are true, the operations defined as addition and multiplication of probabilities have the properties possessed by arithmetical addition and multiplication; there is therefore some justification for giving these operations these names. If the laws are not known to be true, the justification of these names for the operations is more questionable. If different names and different notations be used for these processes, what appears to be a serious fallacy in the whole of the treatment of the fundamentals of probability by Mr. Keynes becomes very apparent. If, for example, the probability $\frac{ab}{h}$ is defined as the *iterate*, instead of the product, of

the probabilities $\frac{a}{bh}$ and $\frac{b}{h}$, and the notation used be $\frac{ab}{h} = \frac{a}{bh} \parallel \frac{b}{h}$, instead of $\frac{ab}{h} = \frac{a}{bh} \cdot \frac{b}{h}$, then the following argument becomes clearer: such a change in a mere name and notation can obviously not affect any valid reasoning.

The probabilities $\frac{ab}{h}$, $\frac{a}{bh}$, and $\frac{b}{h}$, are the probabilities of certain statements on certain evidence: given a , b , and h , each of the probabilities is perfectly definite and determinate; each has thus a certain degree, and may easily have a definite numerical degree. Suppose these numerical values, in any particular case are P , p , and q . Then, simply because we have chosen to give the name 'multiplication' to the operation by which $\frac{ab}{h}$ is derived from $\frac{a}{bh}$ and $\frac{b}{h}$, it does not follow that $P = pq$. It is plain, however, that, if we call the operation 'multiplication,' we shall be in danger of committing this error, for, having proved that one probability is the product (*i.e.*, iterate) of two others, we shall be liable to suppose that we have proved that its numerical value is the arithmetical product of the numerical values of the others. If we do this, we shall have unconsciously assumed another law of probability: that, if we have three probabilities of the forms $\frac{ab}{h}$, $\frac{a}{bh}$, and $\frac{b}{h}$, and their numerical values are P , p , and q , then $P = pq$.

There is no doubt whatever that Mr. Keynes does this. At first, he is found talking of the multiplication of proba-

bilities, as defined by him; later, he is found multiplying arithmetically the numerical values of probabilities, and the only justification is that the probabilities are of the form of those in his definition. As an example of this kind of transition, we may take his method of establishing his inverse formula.

From the definition of multiplication

$$\frac{a_1}{bh} \cdot \frac{b}{h} = \frac{a_1 b}{h}$$

similarly

$$\frac{b}{a_1 h} \cdot \frac{a_1}{h} = \frac{a_1 b}{h}$$

therefore

$$\frac{a_1}{bh} \cdot \frac{b}{h} = \frac{b}{a_1 h} \cdot \frac{a_1}{h}$$

similarly

$$\frac{a_2}{bh} \cdot \frac{b}{h} = \frac{b}{a_2 h} \cdot \frac{a_2}{h},$$

therefore, says Mr. Keynes, by division, it follows that

$$\frac{a_1/bh}{a_2/bh} = \frac{b/a_1 h}{b/a_2 h} \cdot \frac{a_1/h}{a_2/h}.$$

If this reasoning is followed in the alternative notation, it will be obvious that the last step in the argument is invalid.

To begin with, $\frac{a_1}{bh} \cdot \frac{b}{h}$ is not of the form $\frac{a_1/bh}{a_2/bh} \cdot \left(\frac{a_2}{bh} \cdot \frac{b}{h}\right)$, so that, if by division is meant the inverse operation to the operation defined as multiplication, it does not follow that $\frac{a_1}{bh} \cdot \frac{b}{h}$ divided by $\frac{a_2}{bh} \cdot \frac{b}{h}$ gives $\frac{a_1/bh}{a_2/bh}$. If division means arith-

metical division, then it has been assumed that $\frac{a_1}{bh} \cdot \frac{b}{h}$ divided by $\frac{a_2}{bh} \cdot \frac{b}{h}$ is $\frac{a_1/bh}{a_2/bh}$; this is only true if $\frac{a_1}{bh} \cdot \frac{b}{h}$ is the arithmetical product of $\frac{a_1}{bh}$ and $\frac{b}{h}$, and $\frac{a_2}{bh} \cdot \frac{b}{h}$ the arithmetical product of $\frac{a_2}{bh}$ and $\frac{b}{h}$. Thus the whole proof of the inverse theorem is based on the kind of fallacy discussed.

I do not mean to say that I consider the inverse formula to be incorrect. Actually, I believe it to be an advance on most previous inverse theorems, and Mr. Keynes applies it very ingeniously to the solution of problems in the reliability of testimony. But Mr. Keynes has not proved it to be true, and I do not think that the psychological theory is capable of proving it to be true. Similarly, the whole of the laws of

multiplication and addition of probabilities, and the fields of applications of these laws, are completely outside of the province of psychological probability as developed by Mr. Keynes.

There seems to be one line of escape from these conclusions.

It is to deny that the probability $\frac{ab}{h}$ is definite and determined apart from the definition of multiplication. It may be held that this probability is not the probability of a definite proposition on definite evidence, but that it is the probability of two simultaneous propositions on definite evidence. It may then be said that, while we have a direct perception of the degree of probability of a single proposition on certain evidence, we have only an indirect perception of the degree of probability of two simultaneous propositions; and that the degree of $\frac{ab}{h}$ is actually only judged by the rule that it is

equal to the arithmetical product of the degrees of $\frac{a}{bh}$ and $\frac{b}{h}$. Thus the relation becomes, not the definition of a product, nor that of multiplication of probabilities, but the definition of the simultaneous probability of two propositions. Its justification would then be that this is actually how we do estimate the probability of two propositions simultaneously; a justification similar to that of the Principle of Indifference.

It seems to me that an attitude of this kind would not be very far wrong; but it would lead to a profound modification of the groundwork of the psychological theory. What is a single proposition? and how can it be recognised of what simple propositions a complex proposition is composed? Any proposition can be expressed as the simultaneous statement of two or more propositions, so some kind of criterion would be necessary to exclude such arbitrary subdivision of propositions. I think that, if the attempt be made to formulate such a criterion, it would be realised that the criterion must ultimately rest on frequency considerations.

It is therefore worth considering whether the frequency theory can be modified and restated, so that it ceases to be subject to the criticisms brought forward against it.

The first and most important difficulty, to explain what can be meant by the frequency with which an event occurs, has been disposed of by Dr. Norman Campbell. Briefly, and, of course, loosely, his conclusions may be summarised as follows:—

In certain experiments, in which the results are due to chance, such as spinning a coin, throwing dice, games of

chance, etc., it is found that each result of the experiment, provided that there is a finite number of possible results for the experiments, occurs with the same limiting frequency in *any* long series of repetitions of the experiments. This limiting frequency is called the chance of the result appearing in the experiment. Thus it is a definite physical quantity capable of measurement. It is in no way dependent on what we know, or on what we believe, but is a function of the physical nature of the objects used in the experiments, and of the nature of the experiment. A typical case is the spinning of an ordinary coin: here heads appears in one half the total number of spins, so we say that the chance of heads is $\frac{1}{2}$.

Mr. Keynes has discussed the hypothetical case of a coin which has been spun 999 times, and has come down heads each time. This, he says, would render it very probable that heads would appear at the next spin, since we now have additional evidence, which makes it very probable that the coin is a fake one, with heads on both sides. Consequently, the probability of heads is very nearly 1. Nevertheless, he would say that the probability of heads at the first spin was really $\frac{1}{2}$, for then we had no reason to expect heads rather than tails, or vice versa. The probability of heads, therefore, started at the value $\frac{1}{2}$, and rose steadily towards the value 1.

The chance of heads occurring, as defined by Dr. Norman Campbell, remained constant throughout all the spins. If the coin were a fake, the chance of heads was 1 at each spin, including the first; if the coin were normal, then the chance of heads was $\frac{1}{2}$ throughout all the spins.

Now, there plainly is some quantity associated with probability, which does remain constant, like Dr. Norman Campbell's 'chance,' and there plainly is some quantity which varies in the way described by Mr. Keynes. These two writers are therefore talking about different things. We have already discussed Dr. Norman Campbell's 'chance:' there remains the question, what is Mr. Keynes' 'probability' which varies in this manner? To answer this question, we must appeal directly to our knowledge of when the term probability may be used. I think that it will be generally admitted that one probability, which varies throughout the spins, is the probability of any theory which we may form about the way in which the coin is constituted. The evidence at our disposal becomes more complete, cogent, and compelling, and it is to be expected, therefore, that any theory based on it should more probably be true.

In the case of the coin here discussed, we have two alternative theories; that the coin has two heads, and that the coin

is normal. If the former be true, the chance of heads is 1, if the latter be true, the chance of heads is $\frac{1}{2}$. But neither theory is known to be true: according to the available evidence at the end of the spins, the first theory is very probable, and the second is very improbable, so that it is very probable that the chance is 1, and very improbable that the chance is $\frac{1}{2}$. From these considerations, it is possible to abstract a new conception, which may be called the 'probable chance' of heads, the value of which is very near to 1, and gets closer and closer to 1, as the number of spins increases. This, it seems to me, is Mr. Keynes' probability.

If this analysis is correct, 'probable chance' is not a fundamental variety of probability, but is derivative, being an abstraction from the probabilities of alternative theories, and from the magnitudes of certain hypothetical chances, on the supposition that the theories are true. Thus its treatment should be postponed until after that of chances, and until after that of the probability of theories. These two, however, are fundamentally distinct types of probability; one varies with our knowledge, and the other does not. It is therefore worth noting that most of the criticisms of the Frequency Theory are concerned with instances in which the probability of some theory is involved.

This subdivision of the subject matter of probability into two distinct branches is by no means a new idea. It has been held in various forms by many writers, from Hume onwards, and Cournot makes it one of the central features of his treatment. Yet it has not always been realised by upholders of frequency theories, and this, no doubt, accounts for many criticisms of the Frequency Theory, which fall to the ground if the distinction be realised. Nevertheless, Mr. Keynes actually discusses Cournot's views on this question and rejects them, but his chief grounds for so doing are really an objection to Cournot's analysis of objective probability. Yet his other criticism must be admitted, that to take up this general view of the separation of chance, or objective probability, from the probability of theories, means that the Frequency Theory can only be applied to a limited region of the theory of probability. This would be a valid objection to the whole theory if there were any alternative, which could not only do the work of the Frequency Theory, but also had a wider scope. This, I have tried to show, is not the case.

There still remain, however, some objections to the Frequency Theory, which apply even when there is no question of the probability of a theory being involved. Thus Mr.

Harold Jeffreys and Miss Dorothy Wrinch,¹ taking, as example, the series of numbers

1, 0, 1, 1, 0, 0, 1, 1, 1, 0, 0, 0, 1, 1, 1, 1, 0, 0, 0, 0, . . .

point out that the frequency of 1's does not tend to a definite limit as the series is extended. It does not, therefore, follow that, in a series of experiments, the frequency of any result will tend to a limit. Yet it is in terms of such a supposed limit that the Frequency Theory defines the 'chance' of an event occurring.

The obvious reply is that the distribution of 1's and 0's in this example is governed by a law, and is not 'due to chance'. It is only when the result of the experiment is due to chance that the limiting frequency is stated to exist, and is called the chance of the event occurring.

This answer immediately raises the question: What is meant by saying that the results of an experiment are controlled by chance, or by equivalent phrases? There are really three questions involved: What is actually meant by saying that chance is present? How can we know when chance is present? and, What kinds of events are those in which chance is present?

There is very little doubt as to what is actually meant by saying that chance is present. It is simply that there is no law uniquely pointed out by the results in the past, from which the individual results in the future may be predicted, while yet each result appears with a definite limiting frequency. Dr. Norman Campbell has pointed out that it is not merely sufficient that each result should have a definite limiting frequency, without other conditions also, for consider the following sequences of numbers:—

1, 2, 3, 1, 2, 3, 1, 2, 3,
 1, 2, 3, 3, 2, 1, 1, 2, 3, 3, 2, 1,
 1, 2, 3; 1, 3, 2; 2, 3, 1; 2, 1, 3; 3, 1, 2; 3, 2, 1; 1, 2, 3;
 1, 3, 2; 2, 3, 1; 2, 1, 3; 3, 1, 2; 3, 2, 1; and so on, repeated.

Here the frequencies of all the alternatives are equal, and in the third case, although not in the first or second, the frequency of any one alternative, say the figure 3, is the same in the whole series as it is in the sub-series of figures following another 3, or following a figure 1, or a 2. Yet, in none of these cases, would anyone judge that the distribution was random, or chance; and if, in such a distribution, he did not at first realise that the distribution was given by a law, so that he came to the conclusion that he had to do with a case

¹ "Some aspects of the Theory of Probability," *Phil. Mag.*, Dec. 1919.

of chance, then, when he did come to realise that there was a definite law operating, he would immediately say that he had been wrong in thinking that chance was present. He would not say that chance had been present up to the time when he had found out the law of the phenomenon, and that then it had ceased to be present. Many writers have, however, explicitly, or implicitly, adopted a view of probability in which this would be the case. The reason for so doing was nearly always the desire to make the statement that chance was present mean something such that it would be possible to find out whether chance was present in any given case.

I do not think that it is ever possible to decide with absolute certainty whether chance is present or not: however, I think that the order of uncertainty which may be reached in some cases is very nearly as small as the order of uncertainty that the sun will rise to-morrow. In such experiments as throwing dice, the frequency of any alternative result is found to tend to a limit, and this limit is found to be the same in the whole series as in any special sub-group, selected according to some simple law. It is conceivable that, in spite of this, the different results really followed some complex law which, like the third example constructed above, gives results of an apparently chance nature without being a chance distribution. It is conceivable, also, that this law might later be discovered, so that, given a certain number of results in previous spins, one might calculate the result in any further spin. But, although it is not possible absolutely to disprove such a hypothesis, I do not think that anyone would seriously believe it, any more than anyone would believe that the sun will vanish to-night, and never reappear; though this theorem is also incapable of absolute disproof. Some reasons why we are so sure that certain experiments have their results controlled by chance will be given later.

This analysis of what is meant by a chance distribution differs from that of Dr. Norman Campbell. According to him, also, a chance distribution is one which is not given by a law, but for him the most typical random distribution is that which would be produced by a conscious being deliberately trying to produce a distribution not given by a law. Other distributions are recognised to be random if they are of the same kind as would be produced by such an individual.

There are two criticisms which may be made of this view. Distributions not deliberately designed by someone to be random may actually be random; in such cases, they can only be judged to be random by means of a judgment that

the distribution resembles a designedly random one. This judgment can only be made in terms of frequency: it must be of the form: "A designedly random distribution has such and such frequency characteristics; the present distribution has these characteristics also; therefore this distribution is to be judged to be random." It is plain that the designedly random distribution need never be referred to; a distribution is random, when it exhibits the frequency characteristics mentioned. Moreover, it may be questioned whether the designedly random distribution is the most typical: to assume that it is, is virtually to say that, if a man is unconsciously arranging the results according to some general law, he will always recognise this tendency, and will counteract it. This is a doctrine on which I should like to hear the opinions of a good Freudian.

Thus, even in Dr. Norman Campbell's test, judgments that chance is present really rest on judgments that certain frequency characteristics are present. The actual criterion as to whether a distribution is random, is whether the frequencies of the various results, and groups of results, obey the laws of multiplication of probability for independent results. It is impossible to know this with certainty, so it is assumed as a scientific theory (in Dr. Norman Campbell's sense of the term) that, if a distribution occurs naturally, and is not constructed by some conscious being, and is such that, when reasonably rigorous tests are applied, it appears to be a random one, then the distribution is actually a random one. It is only by means of such a theory that any distribution can ever be judged to be random.

There remains the question, what events are subject to chance? This is actually the question which most writers have given answers to under the heading: what is chance?

Perhaps the best known answer to the question is that of Poincaré, according to which chance is present in those happenings in which a small change in the conditions makes a very large change in the result. Dr. Norman Campbell, however, points out that this does not ensure the presence of chance: if there are only two alternative results, and one will occur if a certain quantity is greater than a given standard, while the other will occur if it is less; then the results will only be chance, if the distribution of the magnitudes of the variable quantity, above or below the standard, is a chance one. This conclusion is quite unaffected by the magnitude of the change in the result, or by the smallness of the change in the cause. Thus Poincaré's conditions are not sufficient.

Dr. Norman Campbell seems to believe that, for the results to give a chance distribution, the causes must also vary in a random manner. There is therefore, in most cases, no means of telling whether chance is present, except by analysing the results. If this were true, then the Frequency Theory would have to be abandoned, for Mr. Keynes has pointed out that we frequently do judge chance to be present, without any knowledge of the statistics of the events involved. For example, if a new game of chance is introduced or invented, we are often perfectly prepared, not only to decide that it is a game of *chance*, but also to estimate the probability of a specified event, and to bet on our estimate. Mr. Keynes believed that this shows that our estimates of probability are not based on frequencies; but this is not necessarily so. In just those cases where we are prepared to estimate probabilities, we are also prepared to prophesy frequencies.

The fact that we do pretend to prophesy frequencies is of great interest, and an examination of it takes us straight to the heart of the problem. In Mr. Keynes' view, which, on this point, must seemingly be followed by other psychological theories of probability, we can prophesy the frequency because we know the probability, and we know the latter by the Principle of Indifference. He says (p. 334): "Bernoulli's Theorem supplies the simplest formula, by which we can attempt to pass from the *a priori* probabilities of each of a series of events to a prediction of the statistical frequency of their occurrence," and he always uses the result of the Theorem in this sense. Now, an *a priori* probability is, for Mr. Keynes, a concept of the nature of a *degree of expectation*. It would therefore be extremely surprising if Bernoulli's Theorem could, from this basis, really calculate *the frequency of events*. As a matter of fact, it does not do so, and Mr. Keynes, when expounding it, only makes it calculate the frequency most to be expected. It is only by an illegitimate unconscious transition from this concept to that of the frequency which occurs, that Mr. Keynes' subjective probability is made to have any relation to actual frequencies. Even if the theorem is only stated to calculate the frequency most to be expected, it remains a question why this should turn out to be the frequency which occurs. Why should Nature do what I expect? The only possible answer is that my expectations have previously been based on the course of Nature: that the Principle of Indifference has unconsciously been based on experience of frequencies.

If this is so, we must look elsewhere for the explanation of

our readiness to prophesy frequencies. I think it is fairly clear that we only attempt to do so when we recognise that chance is present, and that we recognise this by observing that certain conditions are present, which are sufficient to ensure the presence of chance. What these conditions are has been stated by Mr. Keynes, in his answer to the question: What is objective probability?

His answer is that objective probability is present if we know that we cannot calculate the result of any individual experiment, and could not calculate the result, even if we had perfect calculating ability, and possessed full knowledge of the objects used in the experiments and of the laws involved in the course of the experiments.

Although this seems perfectly accurate, the analysis may be carried deeper. Plainly it is not our inability to calculate the results which causes heads and tails to fall with equal frequencies when a coin is spun. It is therefore clear that objective probability has characteristics other than our inability to calculate the results; and it is clear that these characteristics are in themselves objective. It is natural to suppose that it is in the nature of these objective characteristics, that we shall find the reason why we cannot calculate the results. Conversely, therefore, if we find the reason why we cannot calculate the results, it is to be expected that, in so doing, we shall light on the objective characteristics of objective probability.

In the case of the spun coin, we may know full particulars of the size, weight, and shape of the coin, and know all the laws of impact, motion, friction, etc. involved in the spins; we are nevertheless unable to calculate the result of any one spin. In order to perform the calculation, we should also have to know the exact velocity of the coin in magnitude and direction, the amount of spin imparted, the direction and strength of all wind currents, and full particulars of the nature of the surface on which the coin lands. These we do not know, and they vary from spin to spin. That is why we cannot perform the calculations, and that is the objective characteristic of this chance event.

We can now try to express this result more generally. It is difficult to make the ideas precise, but the following are put forward tentatively as being conditions of the right kind.

If, when an experiment is repeated indefinitely,

- (1) A finite number of different results are possible;
- (2) The result of each experiment is a function of a large number of variables;

- (3) These variables are singly independent, in the sense that no one of them is a function of one only of the others;
- (4) The variables are all liable to change in magnitude from one experiment to the next, within a finite range of variation;
- (5) A change in the value of any one variable, within its possible range of variation (*i.e.*, a change not greater than sometimes occurs), would be sufficient, in at least some of the experiments (in association, maybe, only with particular values of the other variables) to alter the result of the experiment:

then the results of the experiments will be a random distribution of the alternatives.

This means that each result will have a limiting frequency, which will be independent of the nature of previous results.

It will be noted that Poincaré's condition is not used. From the present point of view, Poincaré's condition is not absolutely necessary; if it holds good, then we have an experiment which is sensitive to small changes in conditions, so that condition number 5 is more easily satisfied: that is why Poincaré's condition is very often satisfied when chance is present, although the condition is neither sufficient nor necessary.

The statement of the above conditions, and of the consequences that ensue when they hold good, is a purely objective generalisation. It thus purports to be a law of Nature, in exactly the same sense as the law of gravitation is a law of Nature, for both say that when certain objective conditions are satisfied, certain objective consequences will follow. We will therefore refer to it as the *First Law of Chance*.

It is because we know, in many cases, that the results are controlled by many separately varying factors, that we are so sure that apparently chance events are really chance events. Even if we discover what appears to be a law governing the results for a certain number of experiments, we may sometimes be quite convinced that the law will not hold good for much longer. Our experience is that laws hold good, if the controlling conditions are constant. If we know that these conditions are varying under the influence of other conditions, which are so complex that, in order to allow for them, it would be necessary to take account of almost every particle of matter in the universe, then we may be sure that the conditions are not of the kind which give rise to a law; for the conditions which do this are essentially those in which only a small number of factors influence the results appreciably.

In many cases of chance events we do know that there are thousands of ways in which small changes may creep into the conditions, and that there is nothing to guarantee that these changes will be of the kind required to repeat the results given by the previously found law.

Why the First Law of Chance should hold good it is very difficult to say. The only discussion I know of which throws any real light on the subject is due to Prof. R. A. Sampson. It deals with the way in which an analogous, if not equivalent, frequency law (Gauss's Law of the Frequency of Errors) may arise, simply from the interaction of a large number of separately varying factors.¹

Although the First Law of Chance enables us to know that in a new game of chance, the frequency of each result will tend approximately to a limit, yet it does not give any idea of the magnitude of the frequency of any one result. We have now to examine the grounds on which estimations of such frequencies are actually made, and correctly made.

Suppose that throws are made with perfectly symmetrical dice: it is found that the limiting frequencies of all the sides are equal. If we did not know this for a fact, but adopted it as a hypothesis, and then mentally compared the throws with similar throws with a loaded die, loaded so as to bring the centre of gravity nearer to the 'one' face, then we could prophesy that in the loaded die the frequency of sixes would be greater than the frequency of sixes in the unloaded die. The reasoning would be almost wholly physical, for we can see that, in many throws, the die would, in its last movements, roll over on to some face different from that on to which it would have rolled if unloaded. In the long run, these alterations would favour those results in which the centre of gravity was lowered, *i.e.*, it would increase the frequency of sixes. The loading constitutes *bias*.

It is generally possible, in any particular case, to recognise whether a given condition constitutes bias; it is not, however, always easy to see whether the bias is favourable or unfavourable. A general definition of bias is not an easy thing to give, but I think that the following is on the right lines.

¹ "On the Law of Distribution of Errors," *Proc. Vth International Congress of Mathematicians*, 1912, vol. ii., p. 163.

"The Genesis of the Law of Errors," *Phil. Mag.*, Oct. 1918, p. 347.

These contributions were criticised by Prof. F. Y. Edgeworth, chiefly on the grounds that they were unnecessary, since Gauss's Law of Errors can be established without them, by direct deduction from the laws of probability. From the present point of view, however, it would appear that Prof. Sampson's discussion throws light on the origins of the laws of probability themselves.

In an experiment with a finite number of chance results, if one of the factors, on which the result of the experiment is dependent, is related physically, in a special way, to some of the alternatives, then these alternatives are biased.

We can now enunciate the *Second Law of Chance* :—

If there is no bias, the frequencies of all the alternatives will be equal.

If bias is present, the biased alternatives will, in general, have frequencies different from those unbiased, and the difference in frequency will be an ascending function of some physical magnitude in which the degree of bias would naturally be expressed.

In the case of the loaded die, one of the most natural ways, if not *the* most natural way, of expressing the degree of bias would be the amount of displacement of the centre of gravity from the centre of the die. The difference of frequencies of sixes and ones is an ascending function of this magnitude.

It should be emphasised that bias is a physical magnitude, which is capable of measurement: its influence is realised and estimated by the ordinary laws of Nature. Thus, in the case of the loaded die, the influence was estimated in terms of the usual tendency of centres of gravity to become lower. It is for this reason that we can select some methods of measuring bias which are more natural than others. In some cases, especially when the bias is geometrical, it is possible to see that the frequency will be proportional to the bias. These are some of the most important cases in which frequencies may be estimated beforehand. Most of the other cases are where bias is judged to be absent.

Poincaré's paradox is now finally disposed of. It is not because of our ignorance that we are enabled to calculate results such as frequencies, nor is it because the events are not controlled by law; on the contrary, it is because the events are controlled by law, and because we know this law, that we are able to make prophecies which turn out to be correct.

These laws of chance are laws of Nature, not laws of thought or of psychology. Like any other such laws, they may, in time, be superseded by more accurate laws, and they may only hold good in limited regions. For example, it seems that in sub-atomic and quantum physics probabilities, or chances, may obey quite different laws; thus, the chance of a certain kind of change occurring may be directly proportional to some important physical magnitude in the neighbourhood. In fact, a suggestion of this kind has even been made which almost amounts to explaining light as being a wave of probability.

There still remains another important criticism of the Frequency Theory, made by Mr. Keynes, which has not yet been dealt with. It is that probability and frequency cannot be equivalent, for the probability of death within a year of a man of sixty depends, not only on the statistics of such deaths, but also on our knowledge of his state of health.

Here it is necessary to point out a distinction. When we wished to find the chance of a coin throwing heads, a coin was spun a large number of times, and the frequency of heads was obtained from the statistics. The case of the man of sixty is not analogous, for the statistics used here dealt with other men. To obtain an analogous case with coins, one would have to consider statistics of single spins with a large number of coins, bent and worn in all manner of ways. Here every spin is biassed to some degree or other, and it is impossible to apply the results to any other coin, without the intervention of some theory as to how the bias is distributed, and without some supposition as to the degree of bias in the particular case to which the statistics are to be applied.

From this point of view, the whole subject of statistics is really concerned with what may, and what may not, be deduced from statistics of variable bias. It deals essentially with methods of analysing the statistics, with the object of discovering whether the distribution of the bias is a random one, or is itself biassed. Examined from this point of view, the treatment and development which this subject has received, though open to many of Mr. Keynes' criticisms, is, on the whole, more securely grounded than he makes out. In particular, the methods of Lexis and Von Bortkiewicz, analysed by him, and his own developments of them, become more readily comprehensible in the language of variable bias, and are more readily justified and applicable.

These, I think, are the lines on which the Frequency Theory can best be developed, so as to avoid mis-statements and expressions liable to be misleading both to critics and to upholders of the theory.

There remains the whole question of the probability of theories. Since the Frequency Theory, in the form so far discussed, is restricted to objective probability, may it not be that Mr. Keynes' treatment is the right one for the probability of theories? Here, essentially, we are dealing with probabilities which vary with our knowledge, and this was the chief *raison d'être* of his method of developing the subject.

Without wishing to deny the validity, or some special advantages, of Mr. Keynes' method, I think it is necessary to

inquire anew into the foundations of this type of probability, even if the results obtained do not differ to any great extent from those of Mr. Keynes. There are three main reasons for so doing.

One of Mr. Keynes' arguments for his system was that it did lead to 'correct' results in the theory of probability: but these results are only recognised as being correct in the case of objective probability, and I have tried to show that Mr. Keynes does not obtain them in this branch of the subject.

Another difficulty which remains as acute as ever, in the field of the probability of theories, is that of establishing the laws of addition and multiplication of probabilities.

The third point is that the Principle of Indifference now requires justification. I think that enough has been said to make it seem probable that the P. of I., in its applications to objective probability, is really founded on experience of frequencies, and on conscious, or unconscious, realisation of the laws of chance. This, then, would be the source of our trust in the principle, and it might be that our confidence in it, when we deal with the probability of theories, is due to a reflexion of its success in objective probability, and is quite illusory. If, on the other hand, the use of the P. of I were justified, then it might be that, here also, it is equivalent to some kind of frequency theory.

This last consideration is virtually an inquiry whether the probabilities of theories may not be founded on some form of Frequency Theory, *i.e.*, on some experience of frequencies. The following argument tends, I think, to show that it should be possible, in some such way, to found probability on experience.

Suppose someone to exist who had never heard of, and had never formed, even as a hypothesis, any theory which he then, or later, found not to fit the facts. Such an individual could obviously never form the concept 'false,' for he would have no experience of false propositions, from which to abstract the conception. Equally, he could not form the concepts 'true,' 'probable,' or 'improbable'. There is, for him, nothing to differentiate propositions into these classes: all propositions are, for him, simply 'propositions'.

From this we may conclude that the concepts 'true,' and 'probable,' are abstractions from experience; the experience, not so much of propositions which are known to be true, but of propositions which have been discovered to be false. It is true that there must also be experience of propositions not of this type, but I do not think there need be experience of propositions known to be true, before the concepts can be

formed. In view of the controversies over the nature and meaning of truth, it would indeed be hard if we had to wait for the experience of propositions known to be true, before we were able even to form the concepts 'probable' and 'improbable'.

I believe, not only that an alternative type of classification of propositions by results can be found, experience of which *could* lead to the formation of these abstractions, but also that it is actually experience of such propositions which *does* lead to them. The whole elaboration of these ideas may be described as an attempt to base probability on induction. It is thus an inversion of Mr. Keynes' treatment of induction.

According to Mr. Keynes, induction and probability mutually reinforce each other. They may therefore be regarded as a spiral, one side of which is probability, and the other induction. Each depends on the other for support, but each time at a higher level, so that the whole reaches into regions of comparative validity. Mr. Keynes first approached from the side of probability; here the approach is from the side of induction.

The difference in approach necessitates an analysis of induction and its results, on its own merits apart from considerations of probability; so far, therefore, it will differ from Mr. Keynes'. Nevertheless, the existence of Mr. Keynes' admirable analysis of induction makes it possible to present some of the ideas involved in outline only, as the details may be found in his Treatise.

For the classifying of propositions, a fundamental distinction, pointed out by Harold Jeffreys and Dorothy Wrinch, should be noted—the distinction between descriptions and generalisations. A description is a proposition purporting to state something which has been observed about a finite number of objects. Even if the same statement is made about all of them, it remains a description; thus a law may be a description, if it is intended only to apply to those instances, finite in number, for which it has been observed to apply. As an example, suppose that the distance of a moving body from a fixed point has been observed

to be	0,	16,	64,	144,	256,	400,	576,
at times	0,	1,	2,	3,	4,	5,	6,

then to state that the relation between the distance s , and the time t , is given by the law $s = 16t^2$, is a mere description of what has been observed.

Suppose, however, that the law is intended to apply to all intermediate times, so that one could infer from it that at

time 2.5, the distance was 100, then the law is no longer being used as a description, but as a generalisation: it is a statement made about instances which have not been observed as well as about those that have. It follows that we can never know a generalisation to be true, in the sense in which we can know descriptions to be true. All scientific theories, and laws, are generalisations, and it is the aim of Science to replace all descriptions by generalisations; *i.e.*, to replace propositions known to be true by those which cannot be known to be true.

However, it remains true that both descriptions and generalisations can be known to be false, so that, as far as the acquiring of knowledge is concerned, a false theory is a more primitive idea than a true one. Falsehood is thus a more direct abstraction than truth. Truth is essentially a negative concept, and falsehood a positive one, at any rate when we have to do with generalisations: truth is thus an abstraction by negation from the concept 'false'. The trouble is that abstraction by negation is not necessarily unique: there are many negatives to a thing. It is partly owing to confusion between many kinds of negative to 'false,' that some of the disputes of metaphysics over 'truth' are due. What we are concerned with here is one kind of negative, experience of which generates the concepts 'probable' and 'improbable'.

To begin with, we shall ask why some scientific laws are regarded as being more secure, more valid, better established, or as being, in some other way, better than other laws. As we have seen, the answer cannot be that they are known to be true, in the same sense as descriptions are known to be true. It is much more complicated than that.

Scientific generalisations are made by generalising some description, or by deduction from some previous generalisations so formed. Thus, corresponding to every generalisation there exist two classes of instances to which the generalisation is known to apply: the original class on which the generalisation was framed as a description, and the wider class of all other instances for which the generalisation has since been 'verified'. When the law is stated for either class, and not intended to cover cases for which it has not been verified, the status of the law is that of a description. Every generalisation has thus, corresponding to it, two descriptions, the original one, of which it is a generalisation, and the extended one, for all the instances for which it has subsequently been verified. The second one is often very much larger than the first.

Now, Harold Jeffreys and Dorothy Wrinch point out, it is logically always possible to find some law which will be a description of any finite class of observations, and it is always possible to generalise this description. The curious fact is that such generalisation should so often be widely 'verified,' *i.e.*, that descriptions should be so extensible. It is not obvious *a priori* that they should be so; this fact, that descriptions are so extensible, is a fact of Nature, not a law of thought, nor a habit of our minds. It may be expressed by saying that Nature is uniform.

But not all descriptions are so extensible: exceptions are sometimes found to the corresponding generalisations. Nevertheless, it is found that, when this is so, the exceptions generally lead to fresh generalisations. A generalisation may state that when conditions $A_1, A_2, A_3 \dots$ and so on are present, then conditions $B_1, B_2, B_3 \dots$ and so on are also present. An instance may be found in which the A's are present, and not all the B's are present; this contradicts the generalisation, which is thus proved false.

However, the important point is that, in a very large proportion of such cases, it is found possible to produce new generalisations which are not falsified by the same evidence, are in their turn extensible, and are not completely different from the old generalisation. Thus it may be found that, by adding additional conditions A' to the A's, or by subtracting from the B's, or by providing alternatives to the A's or the B's, a new generalisation has been obtained which has been more widely verified than the old one.

As an example, take the generalisation 'small light bodies do not attract each other with an appreciable force,' and suppose that we have defined the terms 'small,' 'light,' 'appreciable,' so that the generalisation is not ambiguous. We find that this generalisation has been verified for a large class of instances, much larger than that on which it was originally based. The conditions, A, are the proximity, the lightness, and the smallness of two bodies; the conditions, B, are the absence of any of the signs of an appreciable force between the bodies. It is found that there are cases which contradict the generalisation; but, if we add to the conditions, A, such conditions, A' , as: the bodies have not recently been rubbed against other bodies, or against each other; they have not recently been connected to the poles of a large electric battery; they are not made of iron, or, if they are, they have not been rubbed by a magnet, and so on; then the new generalisation is not only not falsified, but may be verified for further instances.

The group of restrictive conditions, A', which are needed to save a particular generalisation, are very often the identical conditions which are required to save some other generalisations. They may then be said to form a natural group. Once again, although it is always theoretically possible to find restrictive conditions to modify any particular generalisation, so as to make it include a few cases which had falsified it, yet the fact that these conditions are also suitable for other generalisations is not a logical necessity, but a fact of Nature.

• It may be expressed by saying that Nature is a unity.

The groups of characters of the type A' are taken by scientists to be signs, expressions, or functions of some hypothetical object, entity, or agent, such as electric charge, magnetism, high temperature, atom, energy, libido, etc. One consequence of this is that scientific theories, which state relations between various scientific entities, can logically be reduced to a set of generalisations. Certain groups of characters are 'interpreted' as 'being' particular functions of the hypothetical entities, so that any statement about the latter must necessarily involve statements about the observable characteristics.

(Note.—It is not assumed here, or in any part of this argument, that these views, or methods, of the scientists are correct, *i.e.*, that the scientific entities are real, in any metaphysical sense of the term. Nor is the contrary position assumed.)

But, if several groups of characteristics are interpreted as being different functions of the same smaller group of scientific entities, it is plain that any new generalisation about the latter will involve more than one new generalisation about observable phenomena. Thus it is by no means obvious *a priori* that the interpretations, and the hypothetical properties of the supposed physical entities, which are required to 'explain' one set of generalisations will be consistent with those required for another set. Yet to a very large extent, this is what has been found to be the case.

Once again, this is a fact which is involved in the nature of the phenomena with which we are dealing; it is not a mere habit of thought, nor a logical necessity. This fact is

• another aspect of the unity of Nature.

We are now in a position to say what we mean when we think that some generalisation is better established than another. If a generalisation has been verified for a very large class of instances; if it has been incorporated by a system of 'interpretations,' into a system of generalisations about scientific entities, which has had many other consequences

widely verified; if all the interpretations used have also been used for many other generalisations; and if these interpretations, and these properties, among others, have led to many *new* generalisations, which have been verified; then the generalisation is considered to be well-established.

While it cannot be *known* that a well-established theory or generalisation is not false, it can be known that a theory known to be false cannot be well-established. Thus well-established is one kind of negative to false.

It is a fact of experience that many generalisations have been discovered, or formulated, which may be placed in the well-established class, and also that many have been formed, which may not be so placed. It is found that certain methods of inquiry, certain methods of analysing data, and certain methods of reasoning have more frequently arrived at good generalisations than other methods. What these methods are depends to some extent on the nature of the subject dealt with, but there are some characteristics of the methods which are more or less common to all the fields. An account of these would take us too far into details; they have been widely discussed under the heading 'scientific method' and Mr. Keynes gives a particularly good discussion of them; but it is of interest to point out that as the number of well-established generalisations in physics is increasing, new methods, also, are being found to be successful in arriving at new generalisations. For example, the kind of mathematical analysis, which gave us the theories of relativity, is now being trusted, very largely on its own merits, and is now being developed, in order to arrive at new generalisations.

A new generalisation, which has not been incorporated into the body of knowledge for a sufficient time for it to be ascertained whether it is well-established or not, may, or may not be generally trusted. As a rule, the more of the conditions it satisfies; the fewer, and the less far-reaching, the new 'interpretations'; and the more the methods, by which the new generalisation was reached, were successful in the past, the more the new generalisation is trusted.

The degree of this kind of trust is, I think, a parallel to the probability which the new generalisation is judged to possess. Given a new generalisation, the more *frequently* other generalisations have finally been shown to be well-established, when they were originally formed by the same or similar methods, and when they originally satisfied, to the same, or a similar, degree, the conditions for a good generalisation, the more probable is the new generalisation considered to be.

Thus the probability of a new generalisation is essentially the frequency of success of the methods by which it was reached; where 'success' means success in reaching well-established generalisations. This analysis of the nature of probability has some close affinities with the idea of 'truth-frequency' discussed by Mr. Keynes, save that it is restricted to the probability of theories, and the class of propositions, among which the frequency is being sought, is defined as a limited class, that of generalisations reached by similar methods. Moreover, the idea of truth is not involved: for, although there is no doubt that most scientists would regard a generalisation which is sufficiently well-established as being true; yet, if they did not do so, and even if they considered that truth were not ascertainable, they would still give a greater measure of trust to the methods which reach well-established generalisations than to other methods.

It will be seen that the probability of a theory is a much vaguer conception than objective probability, and that, in general, its magnitude cannot be closely estimated. Moreover, since, in order to derive some idea of its magnitude, it is necessary to compare the generalisation with others reached by *similar* methods, in similar circumstances, the magnitude will have much of the uncertainty attached to a degree of similarity. This is a property which Mr. Keynes found to be present in some probabilities. Nevertheless the definition of the probability of a theory as a frequency, even if it is a frequency whose exact value is unknown, means that the laws of addition and multiplication of probabilities can be established for the probabilities of theories as well as for objective probabilities. It is not immediately obvious, however, that, on this view, the sum of the probabilities of all alternative theories amounts to the number 1. It might appear that a theory was eminently satisfying in all the ways considered, so that its probability might be very high; nevertheless other alternative theories might conceivably be produced which would have as high a degree of probability. I think that this difficulty is only apparent: the frequency of success of similar theories in the past cannot really have been very high, for, if there were alternative theories, as there must have been, only one of the alternatives is finally found to be well-established; so the frequency of success in the whole set must have been low. This shows that, however attractive a new theory may be, it must be judged to have a low probability at first, unless it is almost inconceivable that there should be other alternatives of a similar degree of probability. This is what is the actual attitude of scientists to new theories

in the majority of cases: many delightful theories are continually appearing in scientific journals, but few are immediately judged to be very probable.

Another advantage in defining the probabilities of theories in terms of frequencies, is that it at once points the most natural method of deriving 'probable chance'. If the chance on any one theory is multiplied by the probability of that theory, and all such quantities, according to the different theories, are added, then the result is the probable chance, for it is the frequency with which a man will be right, who asserts that the event will occur, as far as a man who asserts a theory can be said to be right.

It may be noticed that, on the present theory, chance is placed in a less fundamental position than the probability of theories. The fact that a given event has occurred in the past with a definite limiting frequency gives no clue as to what will occur in the future without the intervention of some theory. This, however, does not render chance, or probability, any the less a scientific subject. Chance depends on the probability of theories as much as, and no more than, the law of gravitation. There is only the same kind of doubt, and for the same reasons, attached to prophecies of frequencies as to the prophecies of the Nautical Almanac, although the degree of doubt may differ considerably. Science, in dealing with this subject, as with others, proceeds to find the consequences of its theories. The fact that philosophers may not approve its theories, or may not consider them to be proved as they would like them to be proved, has not deterred scientists in the past from developing their theories as they stand, nor is it desirable that it should. The case is in no way altered by the inclusion of the study of chance in the subject matter of Science.

The probability of theories is, however, far more the affair of the philosopher. Mr. Keynes and Dr. C. D. Broad have independently arrived at a general theory which makes induction and probability two aspects of the same process by which knowledge is extended. They find that, in order to justify the process, it is necessary to assume some such proposition as that Nature is Uniform. The present suggestion is that it is only in terms of this process of thought that the ideas 'true,' 'probable,' etc., arise at all, so that without the assumption necessary to arrive at scientific generalisations, not only is Science impossible, but so is all logical thought. In this way, perhaps, may the assumption be vindicated.

II.—DISTINCTION AND PURPOSE.

BY ALFRED SIDGWICK.

IN a previous article (MIND, vol. xxxiii. no. 132, p. 385), I had occasion to argue that recognised truth—*quâ* recognised—is relative to human purposes. Truth which is independent of human recognition may be harmlessly identified with Reality, but until some criterion is discovered by means of which we can verify our guesses (or beliefs) about the ultimate constitution of Reality it seems premature to claim that any one of them is true. It is possible to combine this cautious attitude with regret for the need of it, and to sympathise with those who try to surmount the difficulty. But sympathy with their efforts does not compel us to admire the methods they use. And in particular two of their methods seem open to serious objections.

The commoner of these consists in assuming that the chief key to success in metaphysics is *strictness* of thinking. The contrast between strict and slipshod thinking has an obvious value in daily life, and most of the difference between science and common sense consists in the greater strictness of the former. Hence it is imagined that there is room for a still more perfect strictness than that of science, and that the metaphysician as such is bound to employ it. The difference, we are told, between the genuine philosophers and others is that the former alone make a determined attempt to bring all their results to the test of perfect consistency; that philosophy, properly speaking, is constituted by this determination; that all other thinking, even that of science, is tainted with mere opportunism and regard for the low ideal of serving practical purposes. One of the best-known supporters of this view was the late Mr. F. H. Bradley. The fact that he did not hold it quite consistently need not surprise us, but that on the whole he supposed he did so is made clear in many parts of his writings.

The other of the two weak methods may also be illustrated from Mr. Bradley's arguments, and it supplies one example of his unintentional departures from his own ideal of strictness. It consists in claiming that an idea which is "true

so far as it goes" is a firm foundation on which to build.¹ There is probably no plausible error to which the faint praise "true so far as it goes" would not apply, since an error becomes plausible just so far as there is something to be said in favour of it. For instance it is true (so far as it goes) that the sun travels round the earth, but to build astronomical science on this foundation is not now thought desirable; and Euclid's notion of Space may also be called true so far as it goes, but the question how far it does go has already led a number of people to doubt some of our most established axioms. The attainment of what we recognise as truth always consists in discovering with increasing precision the *limits* of some 'truth' previously accepted. So that to build upon a truth of which we can only say that it holds good as far as it goes is to build upon an uncriticised foundation. What we always want to know is *how far* a given truth-claim can be trusted.

But the first of these two methods is the more respectable and therefore more effective and dangerous. There is a long tradition behind it, based upon the kind of certainty that mathematics and formal logic provide. To a mind in this static and non-experimental condition it seems axiomatic that we cannot have an excess of strictness in verbal reasoning; that unless we rigidly apply the Law of Contradiction, and deduce our conclusions as strictly as possible, we are not doing all we can. Probably no philosopher has consistently lived up to this simple ideal, and Bradley—as already quoted—has not done so. But a false ideal may mislead us even if we are not at all times faithful to it, and the question remains whether strictness in verbal reasoning is a false ideal or not.

Bradley gives it a leading position in the Introduction to *Appearance and Reality*. Metaphysics, he there says, takes its stand on the desire to think about and apprehend reality. And it merely asserts that, if the attempt is to be made, it should be done as thoroughly as our nature permits. Any opponent of this conception of metaphysics, unless he condemns all reflexion on the essence of things, "allows us to think, but not to think strictly," which "seems equivalent to saying you may satisfy your instinctive longing to reflect, so long as you do it in a way which is unsatisfactory".

But unsatisfactory to whom? Evidently not to this supposed opponent, but only to those who, like Bradley, accept the ideal of strict thinking. As to any who may dispute the value of this ideal, Bradley shows no sign of having troubled

¹ *Appearance and Reality*, p. 159 (1st ed.).

himself about their reasons for doing so. The only opponents he here speaks of as possible are (a) the sham sceptics who are dogmatists in thin disguise, and (b) those who judge the method merely by its lack of results and without attempting to trace this defect to its source. But is there any reason why the inquiry should be limited in this manner? May we not ask *why* the ideal of strict reasoning fails to produce satisfactory results?

And first let us make clear what constitutes "strict thinking". The key to Bradley's idea of it is given by his open reliance on the Law of Contradiction as an applicable rule, and the extent of this reliance by his arguments¹ designed to reduce to absurdity several of the most important notions employed in science: *e.g.*, the notion of Change. The truism that "Anything cannot possibly be anything else" is interpreted by him as saying that anything *called* A cannot possibly *be* not-A; so that the truth of any fact is made to depend upon the definition we give to our name for it. Change, for example, must be impossible because a fact called A must not only deserve that name but deserve it to all eternity. There must be no question raised about possible alterations in our definition of A, and any appearance of change as a fact must never be allowed to shake our 'reasoned' certainty that change is an illusion.

Now in experimental science this attitude of slavery to a definition has long been recognised as a fatal barrier to the progress of knowledge. Instances of it may still occur² but no scientific man at the present day would defend the practice systematically. It is sometimes known as the vice of 'verbalism,' and even common sense usually understands its futility in an argument. When facts are found to conflict with our stiff distinctions it is not the facts that have to give way.

Possibly science and common sense are better guarded than philosophy against verbalism, on account of their greater reverence for 'facts'. Those who are trying to theorise about the causal connexion between pairs of events in Nature are often brought up against the inelasticity of language, the artificial character of some rigid distinction. And whenever a scientific error is discovered—*e.g.*, the too static conception

¹ *Appearance and Reality*, Bk. i.

² The most recent I know of among leaders of science was that of Louis Agassiz, in 1860, referred to in "*Life and Letters of Charles Darwin*," vol. ii., p. 333. He tried to rule out the question whether species were variable, on the ground that immutability is part of the definition of the word 'species'.

of 'species'—the effect of the discovery is to alter in the direction of laxity some definition whose excessive sharpness was previously unsuspected. In metaphysics this safeguard is necessarily less effective, owing to the different character of the problems discussed. But, after all, the use of language is common to all reflective thought, and we can examine the ideal of strict thinking in the light of what we know about the conditions of language.

Prominent among these (as language has hitherto existed) is the rule that between the decisive answers yes and no to a question there is no room for a third decisive answer. That is to say, on the many occasions when we cannot see our way to a clear decision, no answer at all is possible until further explanations are given. The supposed question is found to be ambiguous, and in order to become capable of an answer (right or wrong) it has to be met by a fresh inquiry—usually an inquiry as to the exact meaning in which some word is used in it.

'Strict' thinking, then, requires the use of non-ambiguous words in stating the questions we raise and answer. And anyone who claims that his own thinking conforms to this type thereby claims to know whether the questions discussed by him do in fact escape ambiguity. But this claim is shown to be an uncritical assumption when made by anyone who fails to realise the defect of the Law of Contradiction. Every statement of that law has two meanings, in one of which it is a tautological truism while in the other it is a false generalisation. For instance, it is a truism to say that Nothing can be anything else, and it is a false generalisation to say that Nothing can be different from what it is *called*.

We may here usefully glance for a moment at Bradley's inability to understand the nature of this defect. The law "is proved absolute," he says,¹ "by the fact that, either in endeavouring to deny it, or even in attempting to doubt it, we tacitly assume its validity". But does this meet the objection made by those who neither deny nor doubt the law in its truistic interpretation while they complain that in its other interpretation it is false? What they recognise is that you cannot *apply* a tautological truism without altering its tautological character. An applied rule of any kind has to take the risk of being wrongly applied in any particular case; it becomes a generalisation open to criticism, and in this instance it is a generalisation which is patently absurd. For if everything were of necessity what we happen to call it, how would any error of fact

¹ *Appearance and Reality*, p. 136.

be possible? Did Bradley seriously mean that in denying or doubting the truth of this absurd generalisation we tacitly assume *its* validity? We all assume, of course, that self-contradictory statements have no meaning, and that statements which are even apparently self-contradictory need further explanation before a meaning can be given to them. But so far no one has ever pretended to find a self-contradiction in the statement that things are not always what they are called. We cannot suppose that Bradley intended to argue that this very trite though useful objection to verbalism implies a belief in its own opposite. More probably his confusion is explained by the strict reasoner's confirmed habit of ignoring the risk of ambiguity. That habit stands in the way of his recognising that S may be A for one purpose and not-A for another.

An objector may here inquire whether, in condemning the ideal of strict reasoning, we confess to a preference for slovenly reasoning. William James once carelessly allowed this to be supposed.¹ He imagined an opponent claiming that departure from the ideal of strictness would resolve our intellect into "a kind of slush;" to which James answered "Even so, if you will consent to use no politer word." But the supposed objection deserves censure for its lack of truth rather than for its lack of politeness. It is not true that between 'strict' and slovenly reasoning there is no room for a third alternative. There is, for example, what may be called *elastic* reasoning, distinguished from both the opposite extremes by the greater difficulty it surmounts and the greater care it has to use. A notable feature of it is the open recognition that words are inevitably fewer than the meanings they are wanted to express, and that therefore we must always be on the look out for unexpected ambiguities. But the ambiguities contemplated by it are much subtler than those which the verbalist or the formal logician is able to recognise. They are not confined to 'double meanings' in the traditional sense, but exist wherever a distinction is valid for one purpose and not for another, and where an assertor overlooks the difference of purpose due to the occasion on which the word is used. No doubt it is much easier to ignore this reference to particular assertions; but the temptation to save ourselves trouble does not always conduce to the discovery of truth.

By the method of reasoning which I have here called 'elastic,' how should we deal with the problem of change? It becomes merely a problem in allowing for the necessary

¹ See *The Meaning of Truth*, p. 56.

defects of language. On the face of it the description of anything as A implies that if we also call it not-A the total statement makes no assertion at all. But this is only on the assumption that each half of the self-contradictory statement escapes ambiguity. In the absence of this assumption we are free to suppose that the line between A and not-A may be drawn differently on different occasions—at different parts of any duration or stretch of time, however short. The old Sorites puzzle in any of its forms—say a jug of hot water growing colder—illustrates this state of things. The verbalist would say that anything which is 'hot' can never "by the very definition of that term" become anything else. But if this term is actually used to cover some variations of temperature, however slight, it has already that amount of indefiniteness in it. Who is to dictate to us any 'strict' fixation of its meaning?

All that we seem able to say, therefore, about change is that *if* change is possible there must be ambiguity in the terms by which we try to express the occurrence. But to everyone except the verbalist there is nothing surprising in such a fact. All adjectival terms are notoriously more or less elastic and depend on the context in which they are used. It is at once a virtue and a defect of language—as hitherto constituted—that the same word has to serve different purposes. This condition allows the number of words in use to be of manageable extent, but condemns them to a certain amount of indefiniteness and consequent risk of ambiguity. As long as we have to use language thus constituted the ideal of 'strict' thinking is out of relation to the real difficulties of thought, though it lends itself to the discovery of a number of ingenious paradoxes.

It remains to be asked more particularly what the proposed alternative mode of reasoning amounts to, and how its ideal should be followed. There are several distinguishing features of it that deserve some mention, all of them involving a change in the habits of thought which the traditional logic encourages, and all requiring some care and trouble in application. One of these changes consists in connecting the notion of ambiguity with statements (or questions) rather than with isolated words; with words¹ as used in making a statement (or raising a question) rather than with words as set out in a dictionary. There is, of course, no need to

¹ It is of course also possible to find ambiguity in the form of a sentence, but this is a grammatical affair and is of comparatively small importance. It has nothing to do with the distinction between A and not-A and so with the chief difficulties in science and philosophy.

forget that even the double meaning which often belongs to isolated words may on occasion cause ambiguity, but such occasions instead of being typical are both rare and uninteresting. That kind of double meaning is so well known, and the consequent ambiguity so easily guarded against, that we must not allow it to distract our attention from the much more subtle and dangerous kind that arises from the indefiniteness which is an inevitable condition of the use of words in making any statement. Every statement, every question, must have at least one 'general name' in it, and every general name, as such, is imperfectly definite and therefore liable to cause ambiguity.

Suppose, for example, a libel case which has been whittled down to the doubt whether there was 'malice' in the publication. In order to decide between yes and no a jury may conceivably want more information about the meaning of this word *as here intended* than any dictionary can give. The dictionary definition is, let us say 'active ill-will,' a definition which covers an unspecified number of degrees of activity and malignity. The question therefore cannot be answered at all except in view of some standard which shall suffice for the decision. Till that standard is accepted as suitable the question remains ambiguous. And among twelve jurymen there may easily be a clash of accepted standards.

This simple instance is quoted because a difference in the standards referred to is perhaps the most generally recognised source of ambiguity in a question or a statement. Common sense is very much in the habit of allowing for this difficulty and being prepared to meet it. If a stranger in Oxford Street asks his way to Holborn we tell him to keep 'straight' on and neither party raises a difficulty about the slight divergence from perfect straightness. On the other hand if our purpose were that of making an accurate map these little divergences would become important. Here we have two different purposes, with two different definitions of the word 'straight' corresponding to them; the standard used for directing a stranger is more elastic than that required for making a map.

Another feature of our method is the recognition that uncaused error, if it exists at all, cannot be corrected by any critical procedure. By uncaused error is here meant error which has no plausibility. The only errors we can hope to remove are those which are 'true so far as they go' but which do not go so far as their victims imagine. We assume therefore not only that all 'truths' are likely to run some risk of being misapplied, and so to become errors, but also

that all plausible errors are (theoretically) capable of explanation as being misapplied truths, and allow of no other explanation or remedy. It is here that the notion of purpose becomes useful. In saying that a given judgment is true for one purpose and not for another the word 'application' would serve as well as 'purpose'. Wherever a word A has any indefiniteness in it there is to that extent room for the Sorites difficulty. S_1, S_2, S_3 , may all be A beyond dispute, but as the numbers go on there comes a disputable region, and later a region where S becomes indisputably not-A. Then we have an instance where the judgment that S is A is true in one application (*i.e.*, for one purpose) S_3 and not in another—say S_{20} .

Can it be said that these considerations may apply to the difficulties of science and commonsense but not to those of philosophy? One would like to hear any reasons there may be for thinking so, since language has to be used in any kind of reflective thinking. To some extent elastic reasoning is even now used occasionally in philosophical disputes. That is to say the holders of opposite philosophical views do occasionally discover that each of them has had a vision of something that ought not to be entirely ignored. Every philosophical school or sect puts forward a view which is 'true so far as it goes,' or even which has a good deal of truth in it, but which they perhaps extend beyond its proper limit, or apply to purposes for which it no longer holds good. For example, the school which believes in 'strict' reasoning sees quite rightly the value of its method in cases where ambiguity happens to be absent. But—their opponents maintain—they are apt to extend their view to reasonings which are full of ambiguity; as for instance to the arguments which are supposed to prove change to be an illusion.

The habit of expecting our opponents' views to be "true so far as they go," and then trying to discriminate between the purposes for which they may and may not respectively be trusted, need not be imagined as a complete cure for opposition in general; only as an alleviation worth trying for, and as likely to lead both opposite parties towards an increase of insight. There will probably always be opposition to any view when it is first encountered, especially if its opponents can bring themselves to think of it as an old error in a new disguise. And where the new view has far-reaching consequences involving a change in our confirmed habits of thought the opposition will naturally be harder to resolve even by the discriminative process. One object of this paper is to suggest that our discriminative efforts will have a better chance of success when we can see the ambiguity of a question

as arising out of the defect of one particular word that occurs in it. This method has, no doubt, been in use for a very long time, but has been hampered in its operation by the common failure to realise the ambiguity—and so the limited value—of the Laws of Thought.

To see that some particular sharp distinction, used in a statement, is valid for one purpose but not for another is a step in advance of seeing that the truth of a statement is dependent on its purpose. In a controversy it brings the question down to a more definite point. And the search for the guilty distinction is facilitated by our bearing in mind that no statement (professing to express an assertion) can ever be made without involving directly or indirectly predication. In direct predication the question raised is whether *S is or is not M*. Then if we think it *M* for some purposes but not for others we can put this view forward for discussion. But the statement of a general rule (If *M*, then *P*) or an existential statement (*X* exists) also involve predications if they are to have any meaning. As statements, they require interpretation before they become judgments or assertions. And we cannot interpret the statement of a rule except by thinking of it as applicable to particular cases. The limit of the cases to which the rule is supposed to be applicable is the limit of the *meaning* of the statement. Hence we must conceive some particular cases (real or imaginary) as '*M*' before the question whether the rule is true can begin to be answered. In this sense, and to this extent, the question whether *S* is *M* precedes the question whether (or how far) the rule is true. For example, as noticed above, the rule that "If *S* is *M*, then it cannot also be not-*M*" remains a mere tautology until we imagine it applied in some case of which *M* is predicated. And then the further question is liable to arise whether such case is *rightly called M* or not. Where a change is taking place—*e.g.*, from hot to cold—we have to admit that two (slightly) different cases have an equal right to the name *M*, and that somewhere on the scale there must be cases that have an equal right to both the contradictory names, so that decision between them is impossible. What we have to avoid is the crude assumption that if *S* is *called M* there can be no question as to its being rightly so called.

But the Sorites difficulty is so familiar to us all that it is only philosophers in the chains of 'strict' reasoning who become helpless before it. Any recognisable change can be thought of as due to the gradual addition or subtraction of something too small for language to register in clear-cut words. Material

change, however, is not the only kind. Variation of purpose in general includes this and goes beyond it. The question for what purposes S may rightly be called M, as contrasted with not-M, can only be answered in relation to the purposes conceivable at a given time, and the future may always surprise us with purposes not yet conceived.

Existential statements, we noticed, also involve predication but in another way. Here the actually intended predicate is entangled in what grammar and formal logic would call the subject. The question whether 'S' exists cannot be separated from the question how 'S' is conceived—wherever two different conceptions are possible; and so cannot be separated from the question whether S is M or not. The question as to the 'existence' of God, for example, gets whatever meaning it has from the attributes we imagine as divine. And when we question the existence of the Ether, or of anything else whose existence can be thought of as questionable, the same condition applies. Existence without attributes, existence 'somehow,' is verbally but not otherwise different from non-existence.

Predication being thus involved in every kind of assertion, any doubts about the truth of an assertion may always be resolved into the doubt whether some distinction between M and not-M will bear the strain that is put upon it if the question it professes to answer is to have any meaning. This form of expressing a doubt is often used in everyday matters of dispute, whether casual or scientific, and one does not see any good reason why it should not also be used in philosophy. Evidently there are some influential philosophers who have an extreme dislike of it, though they have not so far directly told us why. Do they, for instance, still maintain that the value of 'strictness' in reasoning has no limit? Or that when we can say of a statement that it is 'true so far as it goes' we can safely use it without further inquiry into the limits of its truth? Do they still maintain that the Law of Contradiction can be completely trusted as an applicable rule? Do they fail to see that statements need interpretation before they can be identified with assertions, or that difference of interpretation makes criticism impossible while it lasts?

It is difficult to believe that any of these positions will be openly defended. More probably their defects are kept out of sight because recognition of them would conflict with old habits of thought. Some twenty years ago a good deal of reference was made to them in the pages of *MIND*, with the only result that those to whom the appeal was then made could never be brought to face the doubts suggested.

Controversial tactics may account for some of the misinterpretation of the issues that were raised, but not for the greater part of it. The confirmed habits of confusing statement with assertion, of ignoring the difference between an applicable rule and a tautology, of connecting the notion of ambiguity only with double meaning of the dictionary kind, must put serious obstacles in the way of understanding what the beliefs are on which our line of criticism depends. If, as we hold, ambiguity is a defect to which all questions are liable, in consequence of the necessary indefiniteness of every predicate term, it seems to follow that no question ever gets a meaning except through some limited purpose which justifies (for the time) our treating a fluid distinction *as if* it were perfectly sharp and clear.

The chief application of this view to philosophical disputes is in helping two opponents to limit the range of their opposite contentions. As an example let us imagine one party declaring that the Law of Contradiction is an axiom so indisputable that doubts of it refute themselves, while another party declares that as an applicable rule it has no value at all. I do not know if the latter party exists, but we may imagine it for the sake of illustration. According to our view both these parties make an exaggerated claim; both suppose that their doctrines hold true for all purposes—*i.e.*, on all occasions of their application—while in fact the purposes for which each of them holds true are limited. If the believers in the Law of Contradiction could recognise that it only has value where the terms used are free from ambiguity, and the other party could recognise that the rule is only worthless where the ambiguity is actually present, the issue between them would be narrowed down to the question whether, in some given application of the rule, the lack of sharpness in the distinction between M and not-M has or has not any importance. When we admit that the sharpness of all distinctions is artificial, the use of any distinction as sharp needs justifying by connecting the artifice with some purpose for which the assumption is harmless. In this way we proceed from the vague recognition that the assertion is “true so far as it goes” to the important and interesting question *how far* it can be trusted. If method of some sort is needed for increasing our knowledge, and if the method of “strict reasoning” is found deficient, one would be glad to learn whether any other method than the one here suggested would be likely to give better results. In the absence of all method it seems open to each of two fallible opponents to claim that he is miraculously inspired, and to leave the other asking in vain for evidence of the fact.

III—THE GENESIS OF APPEARANCES.—I. DISTANCE AND MAGNITUDE.

BY C. A. STRONG.

My purpose in these articles is to set forth an hypothesis as to the manner in which sensible appearances come into being. By sensible appearances I do not mean mere qualities, but qualified objects, seen or heard or felt at a certain time and in a certain place, and presumed to exist.

Such an hypothesis, if it is to be worthy of attention, must be free from any taint of subjectivism or of representationism. I wish, therefore, to declare at the outset that, in my opinion, appearances, in so far as they are veridical, *coincide with existing things*, in such wise that, in them, the things really appear. The mind has, in so far, succeeded in penetrating to the things themselves and in viewing them directly. But, since non-existent things and characters also sometimes appear, the mere fact of a thing appearing cannot be taken as a guarantee of its existence: and we need an hypothesis which shall explain how what appears can sometimes exist and sometimes not exist. What is this function of appearing or awareness, which may uncover the existent, or may hold up a mere show; how are appearances produced?

The hypothesis I suggest is that appearances are *figments*, which may or may not coincide with fact; and that these figments are produced by the co-operation of two factors, *sentience* and *action*. Action, or the way we behave upon occasion of a state of sentience—which state really is *in us*, that is, in the place where the correlated brain-process goes on—causes the state to appear to be outside us, at a greater or less distance and more or less enlarged, and also to appear more simple than it really is; fictitiously altering it in two ways which we may call *simplification* and *projection*. In this manner appearances are produced which, strictly, are illusory. *All* appearances are psychologically of the same nature as illusions.

The theory will be seen to lie midway between neo-realism and critical realism. With the former, we admit that appearances, in so far as they are veridical, coincide and are identical

with real things; and are thus able to maintain that things are really in time and space, as they appear to be. With the latter, we insist that appearances may be, and, in fact, that all appearances are, more or less non-veridical, and, to that extent, other than the real things. Knowing, in a word, is *precarious*; and this hypothesis as to the mode of production of appearances offers an explanation of its precariousness.¹

I.

Fundamental Conceptions.

The hypothesis here proposed is in most respects not new. The only feature of it which can lay any claim to novelty is the rôle ascribed to action or behaviour not only in determining certain of the characters of appearances, but in causing the rise of an appearance at all.

1. *Projection*.—It is an old doctrine of the physiologists that appearances are brought before us by the "projection" of states of mind called "sensations".² To many philosophers this view has seemed to rest on a crude psychology; they have been disposed to question the existence of sensations, in the sense here intended—without which there would be no material for projection and no room for the process. A necessary part of our task will therefore be proof of the existence of projectable sensations.

2. *Simplification*.—One of the original ideas which we owe to Bergson is that the specious present is produced by "contraction,"³ or, as Balfour calls it, "compression"⁴—a process by which the innumerable temporal parts of the most recent interval of time are suppressed, and this interval is

¹ For the main outlines of the philosophical position here defended see my book, *A Theory of Knowledge* (London, 1923). The reader will find a very lucid presentation of the same theory in Prof. Drake's just published volume, *Mind and its Place in Nature* (New York, 1925). On the partial identity of the appearance with the real thing, cf. Prof. Montague's *The Ways of Knowing* (London, 1925).

² Prof. Sherrington, either adopting the current phenomenalist view, or with the aim of restricting his statements to what can be empirically observed, prefers to speak of "projicience". But Dr. Head does not hesitate to say "projection" (symposium on "Time, Space, and Material," *Proc. Arist. Soc.*, supplementary vol. ii., pp. 79, 83). Cf. Dr. C. S. Myers: "I believe that this power of projection, the ability of the self to regard its own change of states as something outside itself, is of far greater importance than is generally supposed" (address on "Consciousness," *Lancet* for 29th Nov., 1924, p. 1109).

³ *Matière et Mémoire*, pp. 30, 64.

⁴ *Theism and Thought*, p. 166.

made to appear as one and undivided. By this process sensible qualities are generated. Holt has argued at length that the simplicity of sensible qualities is in a certain sense illusory, and due to the suppression of a multitude of minute parts—to “condensation” or “fusion”.¹ Contraction and fusion are of course the same process; perhaps the most generally applicable and readily intelligible name for it will be *simplification*. But what is the status of the elements to which this process is applied? And what is the nature of the process by which they are simplified away?

3. *Action*.—Our hypothesis is that simplification and projection are the work of action.

That action is concerned in the genesis of awareness has long been recognised implicitly. Dewey insisted many years ago that consciousness is correlated, not with sensory stimulations alone, but with a process in the entire sensori-motor arc.² Münsterberg explained the unity of consciousness by the synthesis of sensory currents necessary to their producing a movement.³ Many neo-realists reduce awareness to mere action on the part of the organism; but they do not make it clear how action can cause us to be aware. Bergson holds that our everyday vision of the world is determined by utility, *i.e.*, by the needs of action; but does not, I think, explain how action can have this effect.

According to our hypothesis, action causes awareness by simplifying and projecting states of sentience. That is, these states, which really are in the brain, appear to us to be external because we react as if they were external; they appear to be qualitatively simple because we are unable to react to them as other than simple. We have not the pre-formed tendencies to action that would permit us to respond to the finer parts of which they are in reality composed; and we have such tendencies which oblige us to respond as if, *e.g.*, visual sensations were outside the body. Thus the externality and the simplicity are data of action, not data of sense.

If this hypothesis is correct, the characters which simplification produces by omission, and projection by imputation or addition, are artefacts—they are, as we may say, *factitious*. This implies that in a certain sense they are fictitious. They are so, however, only psychologically; in that there is no character corresponding to them in the state of sentience.

¹ *The New Realism*, pp. 308-354.

² *Psychological Review*, 1896, pp. 357-70.

³ *Grundzüge der Psychologie*, pp. 529, 548. Cf. James's article on “The Knowing of Things Together,” *Psychological Review*, 1895, p. 118; reprinted in his *Collected Essays and Reviews*, p. 390.

Epistemologically, of course, they are not fictitious, since they are the sole means by which we apprehend reality.

Externality, the enlargement of visual appearances in proportion to distance, simplicity as to spatial and temporal parts, and even objectivity itself will be found to be factitious and fictitious in this sense.

4. *Sentience*.—Though the appearance is partly factitious, it is not wholly so, since it derives some of its characters from the state of sentience. Indeed, there would be no appearance if there were not a state of sentience to project and simplify. To this state the appearance owes its *sensible* nature. Hence, by eliminating the factitious characters, we may arrive at the element of bare "sense," "sensation," or, as for greater clearness I shall call it, *sentience*.

But here a difficulty presents itself. We should arrive at the sentience only *as a nature present in the appearance*. What justification is there for supposing that this nature is the nature of an existent, which causes the appearance to appear? How can sentience be known to exist, and to be the sort of thing which our hypothesis asserts? Is its assertion consistent with strictly empirical principles?

Yes—but the empirical knowability of sentience is a difficult subject, which cannot be intelligibly discussed until after we have considered the corresponding question of the empirical knowability of physical things. I must ask the reader for a few moments to take the existence of sentience on trust.

Since objectivity is factitious, sentience will contain no relation of subject and object—it will not be awareness of anything, but will be like those feelings which possess us without our attending to them. This is what Bradley means by "immediate experience"; I am not sure whether it is what Mr. Russell means by "sensation". James's "pure experience" and Bergson's "intuition" are approximations to it.¹

For our purposes, it will be necessary to give precision to

¹To the reader who feels doubts as to the existence of sentience, Bradley's discussion in *Essays on Truth and Reality*, in the chapter "On Our Knowledge of Immediate Experience," may be recommended. His term "immediate experience," however, is open to objection, as likely to be understood in the sense of awareness of immediate data, or even in the sense of immediate data without the awareness. This is a danger against which Bradley, I think, insufficiently guards by sometimes speaking of immediate experience as "sensuous" or "sentient". James's expression "pure experience" is open to the same objection; indeed, he himself seems to mean by it immediate data—though he denies that these involve a relation of subject and object! The only entirely unambiguous term seems to me to be "sentience": which, coming as it does from *sentire*, may be plausibly held not to involve a subject-object relation.

the conception of sentience in two further respects, which represent departures from the views of most, at least, of the philosophers just mentioned.

5. *Spatiality of Sentience*.—If we are to understand how, out of sentience, action can generate appearances such as the actual ones—and also to understand the relation of sentience to the brain-process—we must assume that sentience is spread out in the three dimensions of space, continuous through time, and composed of an indefinite number of small parts. It will then be possible to explain the absence of the smaller of these parts in the appearance (of spatial and temporal parts in the case of qualities, of temporal parts in that of the specious present) by the simplifying effect of action—or rather of our inability to react, of inaction.

6. *Thorough-going Realism*.—If sentience is in space, and the portion of it used in seeing is inside the body—as the inner being of the visual brain-process—there is no reason why there should not be (and every reason why there must be) an object, consisting of sentience or of something equally real, at the point in space to which the visual sentience is fictively referred. Thus our hypothesis leads to a thorough-going realism—a realism which regards subject and object as two separate existents. They are two co-equal portions of sentience or of something similar, in the one world of space and time.

If this ontology is not to be dismissed as visionary, I must succeed in showing that, both in its assertion of the empirical knowability of physical things, and in its assertion of the empirical knowability of sentience, it is consistent with sound epistemological principles. Both subject and object must be shown to be possible data of experience.

7. *Knowability of the External Object*.—When a portion of sentience, consisting of a multitude of fine parts in a complex spatio-temporal arrangement, determines an act, it is altered in the above-mentioned ways—simplified and projected—and gives rise to an appearance, which can coincide (but never coincides more than partially) with a physical thing. It can coincide with the thing because it is a mere figment, an entity of logic. If it thus coincides, the thing has become a datum of experience. The very fiction which falsely externalised the state of sentience—making it appear to be, where in fact it was not, outside the body—has brought to light an object existing there. An illumination from the body has fallen on the object, showing it in some respects as it really is. Originally all appearances are taken to be veridical, but we gradually, by the method of coherence, sift out

and reject the non-veridical elements—thus constructing first the world of common sense and then that of physical science.

8. *Knowability of Sentience*.—It will be convenient to take this question in three stages, dealing first with the existence of sentience, then with its nature, then with its spatial and temporal characters.

(1) *Is Sentience Knowable as to its Existence?*—There seems to be no ground for the notion that, when we perceive an object, we must needs be aware exclusively of the object, and cannot be aware in any manner of ourselves; or that such awareness as we have of ourselves is only a perceptive awareness of our bodies. On the contrary, when we see, we are often *aware of our seeing*; when we hear, *aware of our hearing*. This awareness of our seeing and hearing has usually been interpreted as being an awareness of *awareness*. But the difficulty of being immediately aware of our awareness is comparable to the difficulty of grasping, not an object or the other hand, but the act of grasping itself. In truth, what we are aware of, when we are aware of seeing, is not the *act* of seeing, but the visual sentience which is the subject (or the relevant part of the subject) of this act.

The visual appearance which arises by the simplification and projection of this state of sentience is at least as capable of throwing light on the state of sentience as on the external object; and it is often taken as doing so. For instance, when we find light trying to the eyes, or a sound unpleasantly loud, our attention is not occupied exclusively with the external cause, but quite as much with the intolerable state of sentience which this cause has produced in us. We are aware of our seeing or hearing as unpleasant. In listening to sweet music we have the opposite experience: we are aware of a sequence of states of sentience which is pleasant. In fact, in aesthetic enjoyment what enthalls us is the character of our states of sentience—quite literally, we enjoy *ourselves*.

Thus the appearance is at one and the same time an appearance of the external object, and an appearance of the subject or state of sentience. Since the object appears only in the form of simplified and projected sentience, we *cannot* be aware of it without being aware at least implicitly of the sentience; and only a slight shift of our attention is needed to make the awareness explicit.

(2) *Is Sentience Knowable as to its Nature?* If the appearance is an appearance both of the object and of the subject, clearly it cannot be a completely veridical appearance of either. Yet it may be *sufficiently* veridical in each case to

give authentic knowledge. The possibility of direct experience both of physical reality and of sentience depends on the fact that the coincidence is in each case only partial.

Simplification and projection represent alterations of the state of sentience, which, in so far, unfit the appearance for presenting this state truly. But, in generating the appearance, sentience has been altered only in certain respects—as to place, magnitude, and the possession of parts—and has not been altered in its nature. This nature will therefore be given correctly in the appearance. It may be compared to a light which, in falling on an object, does not cease to be of the nature of light. Hence, by abstracting from the fictive externality, we may have the nature of sentience before us truly.

Appearances are spoken of as “sensible”. This is a characteristic common to light, colour, sound, odour, taste, warmth, cold, solidity, weight, and therefore not identical with any of these. *In the sensibleness of appearances the nature of sentience appears truly.*

This may seem a dogmatic assertion. But why should we not exercise, in the case of awareness of the subject, the same “animal faith,” or natural confidence in the truthfulness of appearances, which leads us to assume that the objects we see and touch really exist in space and time? The test of our theory will be its ability to synthesise all the facts of matter and mind more simply than any other.

(3) *How may the Characters of Sentience be Known?*—Though the nature of sentience is given correctly in the appearance—and also, we may add, its “extensity” or spread-outness, its intensity, and its temporal successiveness—the place, size, and complexity of the portion of sentience have been altered by our way of reacting; and the question therefore arises how its true place, size, and complexity are discovered. Its place is discovered vaguely in so far as the appearance is referred to the subject—for sounds and sights, as we have seen, are lived within us, they are not merely contemplated outside. But this awareness of their place, and perhaps of their size, is vague. Is it possible so to redirect our attention as to have an appearance given, showing us the state of sentience not only in its true place, but with its true size and complexity?

I think not. In order to see a distant object, we *must* converge and accommodate for it, otherwise we do not get the sensation; and with the convergence and accommodation there go habits of reaction which inevitably cause the state of sentience to appear as external and enlarged. It seems that

determination of the true characters of this state can only be theoretical. But it is not on that account the less necessary if we are to have a theory of appearance at all; and I shall try to show that the procedure of psychology in reasoning from appearances to "sensations" is exactly analogous to the procedure of physics in reasoning from everyday physical phenomena to atoms.

The nature and epistemological basis of our hypothesis have now been made clear, and we may proceed to apply it to the special case of the visual perception of distance and magnitude. These characters of appearance have been discussed with much acumen by Mr. H. N. Randle in his article, "Sense-data and Sensible Appearances in Size-Distance Perception," in *MIND* for July, 1922, and I desire to take up the question at the point where he has left it. The reader will perhaps recall the striking experiment—that of looking at a printed page while fixating a pen-point held midway between the page and the eye, when the print undergoes an extraordinary diminution in size—by which Mr. Randle seeks to prove that there are no sensations distinct from sensible appearances. I accept this conclusion, in so far as it means that there are no sensations which are observable data (just as atoms are not actually observable data). But if, by means of the assumption of sensations which (like atoms) are not observable, we can explain the diminution in the size of the print; if, still more, we can account for the fact that with a reversed retinal image (and presumably also brain-process) we perceive the object erect and in its true relations, that will be a strong argument in favour of our hypothesis in so far as it maintains that the genesis of appearances involves projection. The rôle of simplification in producing sensible qualities and the specious present will be discussed in a second article.

II.

Are Sensations Observable?

In analysing vision, the first thing that strikes one is that depth is given, so to speak, *in different terms* from length and breadth: length and breadth are coloured, while depth is not. If we take a coloured plane, such as the cover of a book, and rotate it about one edge in a direction away from the eye, the colour shrinks and disappears in exact proportion as the depth increases. Depth, or distance, is a sort of blank. We cannot say that it is invisible, because we see it; but, at least, it is transparent—we see objects beyond or "at" it,

and it opposes no obstacle to our seeing them. How is this strange nature of distance to be explained?

Since distance seems to be actually felt,¹ and yet is without definite quality, it is natural to think first of muscular sensations as the feelings conveying it. The eye-muscles play an important part in vision; they are in a different state of contraction according to the distance of the object; this state is felt by means of muscular sensations: perhaps, then, it is these that give us our sense of depth. But how? The exact *modus in quo* is far from clear. Why should muscular sensations, when added to colour sensations, cause the latter to appear to be outside us? And is it the muscular sensations themselves, appearing outside, that give us that sense of an intervening space?

Properly, muscular sensations are simply feelings in the muscles. As such, they can only be *signs* of distance—like the dimness or the smallness of objects—not in themselves a vision of it. How, then, do they succeed in bringing this vision before us? There seems no more reason why they should appear in space outside us than why the colour sensations should appear there. In adding them to the colour sensations we have only repeated the problem without solving it.

The perception of depth is to some degree at least a product of experience; persons born blind and subsequently operated on do not at first perceive it as we do. It is natural, therefore, to think next of the possibility that it is brought before us by means of mental images. These can hardly be visual images of lengths and breadths; no such images are introspectively discoverable. Berkeley's theory is that distance is perceived by means of images of touch and movement: we imagine the movements we should have to make in order to reach and touch the distant object. But these images, too, are not introspectively discoverable. Nor do they seem to be identical with that character of visual appearance which we call depth. Depth seems to be more specifically visual than this. How can tactile and muscular images, any more than muscular sensations, produce that visible gap between us and the colour, that blank, which is necessary in order that the colour should appear as at a distance?

Since depth is a blank, an entirely different element in the situation, a motor as opposed to a sensory element (though I

¹ Cf. James, *Principles of Psychology*, vol. ii., p. 221 note: "The feeling of depth, of distance, of farness or awayness, does actually exist as a fact of our visual sensibility."

do not exclude the possibility that this motor element may be accompanied by feeling) seems better fitted to explain it—namely, *action*. Our hypothesis is that *the colour appears to us to be distant because we react to it as distant*.

The appearance, as we have seen, is correlated with a process in the entire sensori-motor arc, a process having a motor as well as a sensory part. Whether the motor part of this process is accompanied by sentience, is a question which seems to have been decided by psychologists in the negative;¹ although such an element of motor sentience, fused with the colour, would furnish an explanation of the fact that depth seems to be actually felt. In any case, we have either colour fused with motor sentience, or colour by itself, calling forth a reaction, which is a reaction to the object as at a certain distance. In order to have the sensation at all, we must converge and accommodate the eyes, and this is a first phase of the reaction; this phase *implies* the distance, being an adjustment of the organism to it; and it prepares the organism for ensuing reactions to the object as at that distance, which is a second phase. *E.g.*, if we have converged and accommodated for an object at a distance of ten feet, we are in a state of readiness for advancing toward it, touching it, or otherwise behaving with reference to it.

Our hypothesis, then, is that this reaction and readiness for reaction, this adjustment to the external, imparts to the colour alone, or to the colour fused with motor sentience, the character of being at a distance, and so gives rise to the actual visual appearance. The principle is, that if we are in the habit of reacting as if the sensation were in a certain place, and, still more, if we are organised from the beginning so as to respond automatically in this way, it will inevitably appear to us to be there; even though, in reality, it is in quite a different place. Thus, despite appearances to the contrary, visual sensations may really be all in one plane (or even in a concave surface like the retina or the occipital cortex), and action will project them outward to different distances, according to the place with reference to which we react; and the visual datum will be (to take a common example) that of a crowded street, with people, carriages, and buildings all in different planes.

As the vehicle of the perception of length and breadth is the actual extension of the visual sentience in length and breadth (its depth being lost by simplification); so the vehicle of the perception of depth is the connected tendency

¹ But see Münsterberg, *Grundzüge der Psychologie*, pp. 529-30.

to action, or the motor sentience which accompanies this tendency. I should myself be inclined to believe in the existence of this motor sentience, and to suspect that psychologists have overlooked it because of its fusion with the colour sentience to form the appearance. The argument that depth is actually felt seems to have weight here. But even if we adopt the ordinary view and conceive the motor process as insentient, this account of the vehicle offers a satisfactory explanation both of the "blankness" of depth, and of the fact that depth is so closely attached to colour.

Assuming, if only for argument's sake, the correctness of this hypothesis, let us now consider certain consequences that seem to follow from it. If this is the means by which colour is made to appear as distant, depth is psychologically fictitious. But the fiction is one designed in the providence of Nature to acquaint us with external fact. This was the device hit upon by natural selection for making distant and, therefore, materially absent objects present to our minds. In very early times it was of advantage to animals to act as if certain of their sensations were outside their bodies, because by so acting they adjusted themselves to an object which was really present there. Thus "consciousness" was evolved because of its utility. But it would not have been useful if there had not been really an object at that point: psychological fiction therefore implies epistemological realism as its necessary complement.

Our hypothesis receives confirmation when we pass from the perception of distance to the perception of magnitude. That the enlarged magnitude of objects when seen at a distance is not a datum of mere sense, but is likewise factitious, appears from the fact that a smaller near object may exactly cover and conceal a larger distant object—*e.g.*, a book held in the hand cover and conceal a door; and that an apparently larger object, such as a mountain or a landscape, may be framed in by an apparently smaller object, such as a window. The tendency to react as to a larger object, when the object is at a distance, would explain this illusory increase of size; and if, as I have ventured to suspect, there is motor sentience concerned, this latter, fused with the colour, would explain the fact that the magnitude seems actually to be felt.

When we realise that distance and the enlargement which goes with it are data of action, the temptation at first is to infer that by suppressing or reversing the action, or even by merely abstracting from it, we can recover the sensation in its purity, and have it present with its true characters as an appearance before the mind. In this way we fall into the

fallacy of *observable sensations*.¹ As I myself have until lately been guilty of this fallacy, and as we must be quite clear of it if our hypothesis of projection is to rest on a sound basis, I shall devote the remainder of this section to exposing it.

I imagined, then, that by abstracting from distance and magnitude I could escape from the falsifying effect of action, and directly observe the true characters of sensations. Indeed, the facts of covering and framing, mentioned in the last paragraph, seemed to be a case of such observation. When I hold up the book and notice that it just covers and hides the door, am I not directly observing the equality of two sensations? When I notice that the landscape is framed in by the window, and the window in turn by the rim of my glasses, am I not directly observing that the window-sensation is larger than the landscape-sensation and that the rim-sensation is larger than either? James's well-known example of the after-image projected on different backgrounds, and appearing "as big as a strawberry, as large as a plate, bigger than a house," is another case in point. It is natural to infer that in all three percepts there is a sensation, of unaltered size, and that this sensation with its true size has been directly observed.

Now suppose that, while entertaining this notion of observable sensations, we make the acquaintance of Mr. Randle's experiment. This consists, as before explained, of looking at a printed page while fixating a pen-point held midway between the page and the eye. The print will have undergone a diminution in size which Mr. Randle describes as "amazing"; and our confidence in the existence of sensations, with a magnitude unaffected by the distance to which they are projected, will have received a serious shock. There has been no change here (such at least is the assumption) in the size of the impression on the retina: the sensation should therefore have remained unaltered in size. Yet it has shrunk in this "amazing" way. Its miniature size at present is as definitely sensible (*i.e.*, matter of sensible appearance) as the colour. There is evidently no separating the two, and treating the colour as sensible, but the size as illusory. The utmost we can do is to distinguish them, as different aspects of the appearance, and turn our attention either to the one or to the other. It is not to be wondered at if, reasoning from

¹ Some one may ask whether it is not possible to observe a sensation of cold in the feet, a sensation of pain in a tooth. In common parlance, yes: but these are really sensible appearances—in the one case an aching tooth, in the other cold feet.

this experiment, Mr. Randle concludes that there are no sensations—that there are only sensible appearances.

But then how I deceived myself when, in James's example, I thought I observed an after-image that remained unaltered throughout! And, now I consider the matter, I see that I did not observe correctly. I *either* saw the spot as on my thumb-nail, and in that case converged and accommodated for this near distance, with the result of seeing a small object, *or else* projected it upon a wall, with the result of seeing a larger object; I never had these two experiences at once, or was actually conscious of a sensation that was common to them. And now I see that I could not have been conscious of such a sensation. Similarly with the book and the door, and with the landscape, the window, and the rim of my glasses: in the latter instance, if I ever saw the three things at once, the window and the glasses-rim appeared projected upon the landscape and monstrously enlarged in size.

And yet I did, of course, observe the phenomena which I have called framing and covering. But what I observed was a relation between visual appearances that warranted an inference regarding the corresponding sensations: not an actual relation between the sensations.

These considerations suffice, at least, to dissipate the illusion that sensations are observable with their true characters. They prove also that, if distance and magnitude are fictitious, the fiction is one that cannot be reversed or undone at will. There are two reasons for this. On the one hand, we cannot abstain from the reactions which cause the sensation to appear as distant and enlarged, and still have the sensation; we cannot see a distant object at all unless we converge and accommodate for it, and, if we do so, it necessarily appears as distant and large. The only movements we can abstain from or alter, and still have the sensation, are, so to speak, secondary ones—movements of comporting ourselves towards the object when once its distance and size have been fixed. On the other hand, sensation or sentience, as it is apart from projection, is not a possible datum of consciousness—or, to put the point otherwise, it is a possible datum only as projected and enlarged; just as atoms are not observable as such, but only as fused into total objects.

III.

Atoms in Psychology.

If sensations are not observable, why need we assume them? Why not be content with sensible appearances, which are observable?

The question is like asking, why not be content in physics with everyday phenomena—why assume atoms? In psychology, not only are there peculiar relations between visual appearances to be accounted for, such as their framing in and covering each other, their visibility only from the point of view of the body, and their dependence both as to appearing and as to qualities on the brain-process; but there is an even more central fact still, of which a psychology that recognises only appearances can give no satisfactory account, and which requires, if we are to explain it in the same empirical manner in which physicists explain physical phenomena by atoms, the hypothesis of sensations: namely, the fact that all appearances are appearances *to some one*.

This *some one* is, in outward appearance, the organism. Now there are two alternatives between which a psychology whose only data are appearances must choose. It may say that the organism consists of appearances—of other appearances, of course, than the one at any moment given—or that it consists of purely material processes. What, on each of these views, is the conception of appearing or awareness that results?

The former view seeks to regard awareness as inherent in the appearance. This view has two untoward consequences. If appearing is inherent in the appearance, and appearances are identical with real things, these things must continue to be appearances even when they do not appear. And, when they do appear, the only explanation of their appearing that is possible in the circumstances is that it consists in the causal relations by which the object stimulates the organism and the organism responds to the object. Are these identical with appearing or awareness? Awareness means *seeing, hearing*; is seeing or hearing the same thing as these causal relations? Surely not. However closely connected, the act of mind and the physical causation are not identical. The result is that a psychology which denies unobservable sensations, and chooses this alternative, is forced either to make this absurd identification, or to take refuge in the assumption of a mysterious activity of awareness, different in nature from all appearances, and of which it can give no further account. (But why may not this awareness be our projection of sensations?)

If, on the other hand, we say that the organism consists of purely material processes, and that the appearance comes into being only at the moment when the object is perceived, again the same difficulty arises: in what does the relation of this objective appearance to the organism consist? Ap-

parently it can only consist in the fact that a given state of the organism, this purely material existent, is the condition for the rise of the appearance—in other words, in a causal relation. But, again, appearing or awareness is not the same thing as a causal relation. The organism does not merely *cause* the appearance to arise: it *enjoys* it. This, in fact, is why the organism, or the centre of enjoyment in the organism, is called “I”. To relieve the disparateness between the materiality of the organism and the spirituality of awareness, this materialistic view is forced to regard matter as mysterious in its nature and essentially unknowable. (But why may not the appearances which the organism produces throw light upon its nature, and why may not our hypothesis be right that the highest nervous functions consist in whole or in part of sentience?)

Neither of these views offers any explanation of the mode of genesis of appearances. With appearances alone, and without sensations, antithetical to them and real, the task of psychology is reduced to correlating the appearances with processes in the brain. Only by assuming sensations, which, like atoms, are not actually observable can psychology become explanatory.¹

Since sentience must be conceived to be as minutely subdivided as the brain-process of which it is the inner being, what we are here proposing is, in fact, an atomic theory in psychology.

In justification of the analogy between sensations and the atoms of physics, two sets of considerations may be adduced.

¹ In holding that sensations are not observable data, I am following in the footsteps of three eminent philosophers who were also psychologists, and only giving consistency to their doctrine.

(1) No one is more delightfully clear as to the difference of category between “ideas” (appearances) and “psychical states” (sensations) than Mr. Bradley; this being one of the main contentions, against the traditional British philosophy, of his *Principles of Logic*. But, if “immediate experience” contains no distinction of subject and object, it cannot, as such, be an observable datum.

(2) Dr. Ward held (see his *Britannica* article) that feeling and will are “known only by their effects”. In traversing his view that sensations are objective, I am merely bringing them into line with feeling and will.

(3) William James, in the Epilogue to his *Psychology*, *Briefer Course*, came to the conclusion that “states of consciousness are not given facts”. Instead of adopting his objectivistic inference that “consciousness does not exist,” may we not more reasonably conclude that states of sentience exist ungiven, and that the givenness of objects by their means is consciousness?

It may perhaps obviate misapprehension if I point out that, on my theory, sensations are of course observable in the gross, through appearances; but not as (minutely subdivided) sensations. The case is exactly like that of atoms.

(1) Appearances, owing to their mode of origin, are adapted to throw light on the nature of the organism or self, and not merely on that of the object. This was recognised by Spinoza when he said: *Ideae, quas corporum externorum habemus, magis nostri corporis constitutionem quam corporum externorum naturam indicant*. Sights and sounds, though partly coincident with physical things, are not completely so, but contain many elements which are referable not to the perceived object but to the body of the percipient. Thus there are after-images, contrast phenomena, illusory and hallucinatory data (and also, if our account of them as due to simplification is correct, secondary qualities), which cannot except by violence be fitted into the physical world; and which are denied a place in it by physics. Further, there are characters of perceptual data which are intelligible only when we consider that appearances have their source in the body: such as the non-veridical smallness of distant objects, the perspective distortion of lines and angles, covering and framing, and the significant fact that objects are seen only from the point of view of the body. Finally, there is the fundamental fact that appearances arise, cease, and vary in their characters with a bodily process. In view of these various facts, how can it be denied that appearances are adapted to throw light on the self, and that even a self composed of atomic sensations may be required to explain them?

(2) But the atoms of physics, it may be objected, are empirical entities—if not actually observable, they are at least potentially so: whereas these atoms of sentience, by the terms of the hypothesis, are incapable of being experienced, and this hypothesis is therefore a piece of metaphysics in the bad sense. Let us see how much truth there is in this objection.

In thinking of atoms and electrons, we doubtless imagine them as if they were possible data of experience, and perhaps see in our mind's eye minute dots circling on a lighter ground; but we know very well that they cannot have colour (even black or grey), and are consequently not capable of being seen. For us to see them, they would have to be endowed with a colour which they do not at present possess. Notwithstanding this, we consider that they, and they alone, constitute physical reality: that a flower, or a book, consists of them (in a definite arrangement, of course) and of nothing else. They are not merely ulterior phenomena, non-existent at the moment when we see the book: on the contrary, they exist *now*, and are the true account of its constitution.

These two points—their colourlessness, and their present existence—suffice to prove the complete analogy between the atoms of physics and the atoms here contemplated for psychology. The reasons why we cannot actually observe the atoms of sentience are of the same character as the reasons why we cannot actually observe physical atoms: first, because we are built on too large and coarse a scale, with sense-organs unfitted to apprehend anything so minute; and secondly, because the atoms of sentience, considered one by one, have not the qualities which they cause us to experience when, by means of a great cloud of them, we see such an object as a green leaf or feel such an inner state as pain or anger.

Why, if the atoms of physics are not observable, do physicists nevertheless assume them? Because, by assuming them, they are enabled to explain peculiar complicated data of perception which would otherwise be inexplicable. Atoms in psychology will be justified if they can render a similar service: if, by their help, we can explain the genesis of appearances and account for their otherwise inexplicable characters.

Let us now see whether these fine parts of sentience can give us any assistance in explaining the visual characters of distance and magnitude. It is not necessary, for this purpose, to assume bits of sentience as minute as atoms: it will suffice if we conceive them as far smaller than any parts ever discovered by the eye—say, as corresponding in their magnitude to individual nerve-impulses. Of course, in this world of sentience and sub-sentience, there will be the same continuity which we observe between the parts of the brain-process.

IV.

Distance and Magnitude.

The field of vision, if we include in it that blank of distance, forms a sort of cone, with its point in the lens of the eye; from which point a lesser cone, formed by the prolongation of the other, passes to the retina. This is only schematic, since light in fact comes in pencils; but it is as if vision took place only along the central line of each pencil. From each spot in the retina such a line proceeds to a corresponding spot in the visual field; and the interruption or inclusion of these lines is evidently the cause of the phenomena we have called covering and framing. These central lines cross one another in the nodal point of the lens, in such wise that the

retinal image is inverted and converted—or, as we may simply say, *reversed*.

Now the visual appearance differs from the retinal image, and presumably also from the consequent brain-impression (the process in the visual area of the cortex), in three respects: as to (1) distance, (2) size, (3) reversal. The theory of projection by means of action seems at first sight to explain (1) but not (2) and (3). Distance may perhaps be illusory, and due to our habit of acting as if the sensation were at a distance; but why does the sensation appear of enlarged size? Why, still more, does it appear with the arrangement of its parts reversed? Evidently there are two further illusions here (if they are illusions), for which our hypothesis is bound to account. How, by means of a small, reversed sensation which is inside him, can the subject see an object not only outside him, but unreversed and large? Why, moreover, if this reversal is illusory, are we unable to undo it and be aware of the true arrangement of the sensation's parts?

To these questions our atomic psychology allows us to give a clear and satisfactory answer. The answer, briefly stated, is that vision must not be conceived as a function performed by the mass of visual sentience acting as one whole, but by its parts in length and breadth each acting on its own account. Each such bit of sentience, that is to say, incites on its own account to action, and to an action appropriate to itself. Thus it is as if each bit of sentience, the inner being of a nerve-impulse hurrying toward the motor centres and seeking to determine their response, were a little eye, and as if the action to which it incites determined the value, *i.e.*, the external position, of what the little eye sees. Our total vision is the sum of the countless visions of these little eyes.

Any difficulties which this explanation might seem to involve will, I believe, vanish if *all* the following points are borne in mind.

(1) The action referred to as causing projection, and therefore with enlargement and reversal, is not of the deliberate or intentional sort, the sort one can undertake or refrain from at will; but is instinctive, automatic action—the sort one *must* perform in order to attend to and see a distant object at all.

(2) The self which performs this action is spread out in space and indefinitely plural, and the total act is the sum or integration of the activities of the parts. Each bit of sentience has its own special motor action or tendency, and strives so far as in it lies to determine the total action of the organism in its own sense. For instance, each separate bit of a colour sensation tries to bring about an eye-movement

directing our glance to a corresponding external spot; and the bit of colour, therefore, appears to be in the spot towards which our glance, if the act took place, would be directed. It is as if we had as many separate eyes as there are rods and cones, and as if each eye saw along its own visual line.

(3) These lines, as we saw, cross one another in the nodal point of the lens; being identical with the lines which the central rays of the light pencils have followed in coming from the parts of the object to the retina. It is not indifferent that projection should follow out these same lines in the reverse direction, since by that means the spatial positions of the parts of the object are accurately seen: but it evidently involves the consequence that, as the crossing of the light-rays led to reversal of the retinal image and brain-impression, so the projection of the bits of sentience along these lines in the opposite direction must lead to a reversal of that reversal, and therefore to the object being seen erect and in its true relations.

This, I think, is the true solution of the old puzzle about the reversal of the retinal image. It depends, as will be seen, on the three points of (a) projection, (b) crossing, (c) the plural self.

(4) In order, finally, to explain enlargement, *i.e.*, the fact that the apparent magnitude of objects varies with their distance—and, in particular, the extraordinarily small size of the print in Mr. Randle's experiment—we need only remember in addition, first, that the bits of sentience employed in vision are extremely minute: so minute that, if they were projected in fact (like very fine birdshot), a larger and larger surface, according to the distance, would be peppered over with light or colour; secondly, that projection is not an actual but only a metaphorical, intentional, and fictitious shooting-out, a bringing of appearances before the mind and not an actual extrusion of feelings from the body.

Do not covering and framing *prove* that this must be the true explanation of enlargement?

When these four points have been adequately digested, it seems to me that all the difficulties in the notion of the origin of visual appearances by projection from the brain vanish away, and our hypothesis is seen to afford a complete explanation of the facts. If it costs us an effort to believe that characters so unmistakably experienced and so apparently sensible as distance and magnitude can be fictitious, we should remember that vision is a function which has existed since before there were vertebrates, and that an illusion so necessary to its success in conveying knowledge and guiding

action must needs be very deeply ingrained. Are not Nature's ways in producing this function, if our account of them is correct, quite of a piece with the means by which, according to Darwin, she has brought about the origin of species? If, in the laudable effort to give us eyes, she has resorted to a little harmless deception, her lapse must, I think, be pronounced a *felix culpa*.

IV.—VICO'S NEW SCIENCE OF HUMANITY.¹ (I.)

BY T. WHITTAKER.

To write for posterity has been a not infrequent ambition. Giambattista Vico combined with this, in perfect seriousness, the intention long afterwards humorously expressed by Charles Lamb, to write for antiquity.² In natural accordance with these aims, constantly held before himself, his name has remained at once great and isolated. He has been thought to belong in spirit to Greek or Roman antiquity, or to the sixteenth or seventeenth century, rather than to the early eighteenth in which he wrote; and there is no doubt that he prefigured some characteristic ideas of the nineteenth. This is part of the interest of his work; but it is not the whole. He did, as he himself conceived, found an actually new science, not anticipated by any ancient or modern thinker; and, if he was a remarkable anticipator of later ideas, his system comprises also generalised views more adequately thought out by himself than by any of his successors, and reasoned positions corrective of some illusions of the age that was to follow his own.

To show this, I propose to give an account of the *Scienza Nuova* founded on a study of the recent elaborate edition by Dr. Fausto Nicolini.³ The separate treatment of Vico's great work is both desirable and possible: desirable because he himself more than once expressed indifference to the survival of any of his other works; and possible because, whatever the defects of his exposition, as unsparingly animadverted on by Italian as by foreign critics, he has in reality brought his

¹ This phrase, though not in the title of his great work, is used once or twice by Vico himself.

² "La prima pratica è stata:—come riceverebbono queste cose ch'io medito un Platone, un Varrone, un Quinto Muzio Scevola?—La seconda pratica è stata quella:—come riceverà queste cose ch'io scrivo la posterità?" (*Scienza Nuova*, "Idea dell'opera," ed. Nicolini, p. 51).

³ Giambattista Vico, *La Scienza Nuova* giusta l'edizione del 1744, con le varianti dell'edizione del 1730 e di due redazioni intermedie inedite, e corredata di Note storiche. A cura di Fausto Nicolini. 3 vols. Bari: Laterza, 1911, 1913, 1916. Pp. lxxix, 1273 (paging continuous).

"New Science" into a form in which it stands by itself, perfectly clear of every metaphysical doctrine, including even his own occasionally very interesting suggestions.

These may be found adequately set forth in the two standard monographs on his life and thought, by Flint¹ and by Croce.² The most remarkable of them are: (1) an anticipation of Kant's doctrine that we *make* mathematical truth—in Vico's phrase, *verum* is *factum*; (2) a similar but not identical position as regards the science of man. In mathematical truth, according to Vico, there is an element of arbitrariness, since we choose certain abstractions, rather than others, from which to set out. In the science of man, we know from within the subject-matter with which we deal; whence our constructions have a concrete fulness not attainable in mathematical science.³ His method he calls the combination of philosophy with philology; that is, of meditation on ideas with study of the facts of history and pre-history as known from literary records and other monuments, or inferred from the deeper implications of these discovered by comparison of languages, myths and fragments of tradition. Such comparison brings us to the underlying unity (not without difference) of human nature. Man himself makes history, and therefore he can think it and know it.

To go further into Vico's own metaphysical system, which in his minor writings he develops from a theory of knowledge into an ontology, is unnecessary except for very special students. And a rapid indication of the character of his actual method for the science of man will suffice. All students of his work have noticed that he was as much attracted by Bacon as he was repelled by Descartes; and incidentally he claims to have carried over the Baconian method from the things of external nature to the things of human civility.⁴

¹ R. Flint, *Vico* (Blackwood's Philosophical Classics), 1884; translated into Italian, 1888.

² B. Croce, *La Filosofia di Giambattista Vico*, 1911, 2nd ed. 1922. The English translation (1913) has an additional appendix in the form of a lecture on "The Sources of Vico's Theory of Knowledge," not included in the second Italian edition.

For Croce's tribute to Flint's work, see p. 327 (2nd ed.). Flint, not being so far removed in time, found it possible to say a word in praise of Michelet, whose translations (or paraphrases) offend the Italian critics by a style so completely alien to the original. He still remembered that the French historian did more than any one else to carry Vico's fame through France to all Europe in the early nineteenth century. Weber's German translation (1822), which Nicolini praises, was found so unreadable in its literalness that, as he is obliged to admit, the Germans themselves preferred to read Vico in Michelet's French.

³ *Scienza Nuova*, i., 187-188.

⁴ i., 126-127.

He was, in fact, so far as he was methodical, an inductive, not a deductive, thinker; but, unlike Bacon as well as Descartes, he had no hope of formulating a procedure which, simply in virtue of its methodical character, would infallibly yield true results. He was quite conscious that what he had himself achieved was done by the reading of facts in the light of a kind of scientific intuition, not by the application of any formulated rules. This means that he was not in effect wrong in seeking inspiration from Bacon, who preached method but whose own mind was, like Vico's, intuitive and inexact. The affinity comes out in Vico's excursions into the aphoristic mode of statement, as also in many phrases indicating close familiarity with Bacon's thought.

His peculiar scientific intuition was in the first place arrived at by the study of Roman law, the growth of which he came to see as what we should now call an "evolution". That is to say, he saw it as a social product, and explained it by changes in the social factors from age to age. There were not in the beginning codes wrought out by great legislators. The notion of the Twelve Tables, for example, as a definitive document ascribable to particular persons, was a "poetic" fiction of later times. The laws of the Twelve Tables belong in their origin to different ages. They came into being by a slow growth in response to the varying needs of a society that was constantly undergoing modifications. And the same formula did not mean the same thing for successive ages: not only laws but the interpretations of laws became modified. Words changed their meanings: "king," "people," and "liberty" meant one thing at the beginning and another thing at the end of a long political development. So it was in relation to everything. Early societies see the whole social world, external nature and the order of the universe in a way different from the way in which they present themselves to later societies. These differences are not arbitrary. All the phenomena can be reduced to a certain uniformity because they are correlated and change in such a manner that given one set of factors, or one set of antecedents, the other factors, or the consequents, will be the same in the same circumstances. And so we rise to the most general position. "This civil world," says Vico,¹ "was certainly made by men; whence there can be found, because there ought to be found, its principles within the modifications of our own human mind."

His science of man is thus a subjective science as

¹ i., 172-173.

distinguished from the objective sciences of inorganic or even of organic nature. Its facts, that is to say, retain in them, as essential parts, thoughts, desires and feelings. It is also fundamentally a science of human nature conceived as social. This sociality is never simply that of an animal species. Man, when he has once emerged (by whatever process) from the merely animal life, constructs for himself a world of imaginations and memories which, at first fluctuating and predominantly fictitious, pass at length into definable ways of thinking and acting and determinate historical traditions. These become capable of investigation through the continuity of the tradition in each society, preserved especially in its literature and laws. Vico's aim is to bring out the consensus at each social phase, and to establish a definite sequence of transitions from one phase to another. The stages of development, it must be added in order to distinguish his general view clearly from that of his successors, are not those of Humanity as a whole, but of the separate peoples which, becoming by degrees civil communities in the form of cities and nations, manifest in their changes the qualities of a common human nature. This common human nature is indeed modifiable both by innate differences of peoples and by external circumstances; but it has certain "eternal properties" which make it the object of a science. For where there is uniformity science is possible.

Of all this, from beginning to end of the *Scienza Nuova*, Vico has a firm grasp. Where he fails in comparison with his successors is not in insight; for if in positive ideas they are superior to him in some respects, they have also lost from view some things that he saw clearly. His misfortune was to be unable to put his remarkable conceptions into an order that should give the sense of movement from point to point. All the aspects of his thought are always more or less mingled. Now Comte, and even Hegel, resolute reading though they require, always give the impression that there is an end at which we shall arrive, if we follow them step by step. As they had besides a world to appeal to that was better prepared for them, we cannot be surprised that Vico has scarcely come into view in the history of thought except as partially their anticipator.

It is not, however, too late to seek for him an independent standing. The recent movement of philosophical thought in Italy, largely Hegelian in inspiration, may almost be said to have rediscovered him. His fame, for a time extending little beyond Naples and then mainly Italian, is now European. The greatness of his name therefore gives an opening for a

new attempt to set forth intelligibly his far-reaching ideas. In my exposition I shall roughly follow the order of his work, omitting in each section, where it is possible, statements that properly have their place elsewhere. Some repetition will on this plan be unavoidable; but as there is really in the *Scienza Nuova* an implicit logic, I thought the proposed method more hopeful than an endeavour to compress the philosopher's ideas into a framework not his own.

GENERAL VIEW.

With his successors of the nineteenth century, Vico shares the idea of an "immanent teleology" of the human race. While he is himself a theist, his conception of the divine providence manifested in history differs from that which Comte formulates as the "human providence" only in so far as the immanent end is for Comte progress, for Vico conservation. For both alike, all goes on with uniformity according to laws of cause and effect within the world; but their more specific idea of cause within humanity is direction of its course by something equivalent to a general mind that makes use of the purposes of individuals for ends above those that they consciously aim at. Essentially this is Hegel's view also.

Within certain limits, Vico recognises progress in the past; but he does not look forward to any future stage beyond that which has been reached already by the most civilised nations; and he does not recognise any total movement of humanity. Contact between nations as a factor in their movement is admitted occasionally; but the predominant view is that their "common nature" expresses itself in similar movements uninfluenced by the others. This view, in the circumstances of the time, gave Vico a scientific advantage over most of his contemporaries; for he was able in virtue of it to reject totally that doctrine of "the learned" which explained "the religions of the Gentiles" as corruptions of Hebrew religion. As it is only the Gentiles with whom Vico proposes to deal, he is thus able to treat all the origins with which he is concerned as completely non-supernatural; the Hebrews being usually left out as an exception because their religion only was revealed. If he ever touches on their history, it is to find (perhaps inadvertently) resemblances to the "poetic" history of the Gentiles.¹ When he comes to

¹One opinion he expresses is that if there were derivation on either side, it would be more probable that the Hebrews borrowed the names of the letters from the Greeks than that the Greeks borrowed them from the Hebrews: he thinks, however, that the Phœnician origin of all the "vulgar letters" is established. See i., 238-290.

Christianity, he strongly insists on the element it contains of Platonic and Aristotelian philosophy. Of this, which in its generalised form he regards as the ultimately true philosophy, he seeks the origin exclusively in Greek thought; not at all, as some of his learned contemporaries still did, in borrowings from the Jews. In this attitude there was no subterfuge or conscious evasion. Vico was the most open and candid of men and he desired to be a good Christian and Catholic; but, as his biographers have observed, it was not in his power really to care for anything in Catholicism or Christianity except the philosophy incorporated in its historic system. From popular superstition, as Croce puts it, he averted his eyes as if from the faults of a beloved person; and his own philosophy, while the "liberals" have been sympathetic, has always aroused distrust in "less liberal but more coherent Catholics."¹

There is no doubt, however, about the importance assigned to religion in Vico's story of the beginnings of human civilisation. Taking up a saying, which he somewhat transforms in a manner not infrequent with him in quotations, he declares that, as against the opinion of Polybius that if there had been philosophers there would have been no need of religions,² the true view is that without religions philosophers could not have existed; for philosophers can exist only in a political state, and every political state was founded on a religion.

Religions, according to Vico, arise spontaneously from the first imaginations of dawning humanity about the universe. The state of mind which he conceives to be that of primitive men is very much the same as Comte's "fetishism". This "childhood of the human race" is revived in the imaginations of the poets. "The most sublime labour of poetry is to give to insensate things sense and passion; and it is proper to children to take things without sense in their hands and to talk to them in play as if they were living persons."³ This

¹ See Croce, *op. cit.*, pp. 325-326. Flint also finds that in one way "his spirit was far more Greek and Roman than Christian" (*Vico*, p. 48), and describes him as a theological but not a theocratic thinker.

² Vico often returns to the point. What Polybius says amounts really to this: that if it were possible to bring together a political community consisting of philosophers (in the sense of sages) perhaps there would be no need of religion, but that, human nature being what it is, the statesmen of Rome have done well to maintain its practices and beliefs for the sake of the multitude. *ἐμοί γε μὴν δοκοῦσι τοῦ πλήθους χάριν τοῦτο πεποιηκέναι. εἰ μὲν γὰρ ἦν σοφῶν ἀνδρῶν πολίτευμα συναγαγεῖν, ἴσως οὐδὲν ἦν ἀναγκαῖος ὁ τοιοῦτος τρόπος* (vi. 56).

³ i., 133.

poetic or mythological side of early religion, however, is not the only side that he sees. He draws no idyllic picture of a Golden Age in religion or in anything else. He has already the sensitiveness to the darker side of religion that has come to the most modern anthropologists through seeing as its centre not myth but cult. With the nineteenth century in general, he completely rejects the notion that imposture was the source of religions. False religions, he says, did not spring from the imposture of others, but from the credulity of the men themselves who imagined them. It was thus with the sanguinary religions and their principles of sacrifice, which, among the fierce and cruel men of early times, began with human victims. He has no objection to the saying of Petronius, which he often quotes, that fear first made gods in the world. Yet he never quotes without protest the words of Lucretius about the evil to which religion persuaded mankind. It was really the frightful religions (le spaventose religioni), the religions, in various forms, of Jupiter the god of the thunderbolt, that first drove the human race, by the shock of terror, to take the path to civilisation. Thus the "semi-bestial giants," the ancestors, in the post-diluvial age,¹ of the pagan nations, were startled out of a state of promiscuous vagabondage into fixed marriages; whence came, by degrees, settled communities with recognition of private property. Common to all nations of men, from the time when they became men, are religious and matrimonial and funeral ceremonies. The religious practices on which Vico specially fixes his attention are those of augury, of taking the auspices. The gods, it was thought by the first founders of families and of a settled order, intended to say something to them; and augury was the formulation of what was supposed to be revealed. This was the beginning of a human life regulated by law.

There was a certain nobility in these giants on the way to humanity, whose grandiose imaginations founded the Gentile religions. They had a poetry born of ignorance and wonder. It was "divine" from its subject-matter; for what they wondered at they called "gods." We can get some insight into their minds from the things told about the aborigines of America, from what is recorded of the ancient Germans by Tacitus, and from what we observe in children. They at once feigned and believed, with a marvellous sublimity that

¹ Vico's deluge is ostensibly that of the Book of Genesis, but it might equally well be that of Plato or the Stoics. Where he differs is in giving his primitive race more of fierce savagery.

sprang from their robust ignorance, things profoundly disturbing to them, which were brought forth by imaginations immersed in the corporeal. Thus they were poets in the Greek sense of creators. With this creative faculty went the desire for uniformity, expressing itself, however, not in abstractions, but in a kind of fabulous unities.¹ The more robust giants on the mountains, when it lightened and thundered, looked up to the sky ; and, interpreting the effect by their own violent passions, they feigned the heaven to be an animated body, and called it "Jove." Curiosity, the daughter of ignorance and the mother of science, led them to attribute other feelings of theirs to things ; as when they supposed in the loadstone an occult sympathy with iron ; and so the whole of nature became for them a vast animated body which feels affections. This notion of a sympathetic nature as an immeasurable woman we can barely understand and cannot imagine ; even the popular mind being now too much withdrawn from sense by the abstractions of which languages are full, and too subtilised by the art of writing and as it were spiritualised by the use of counting.

While the imaginations of the first men were vast, they were also limited. For the primitive theological poets, Jupiter was no higher than the mountain-tops. It was from thence that he was supposed to give the signs interpreted by the art of divination as his commands. Men called him the Saviour because he did not destroy them with his thunderbolts.

The proper material of early poetry as of all poetry is the "credible impossible". It is impossible that bodies should be minds ; but to early men, not yet capable of abstraction from sensible things, it was credible that the thundering heaven should be a god with mind and will. Man, in the age of metaphysical phantasy which came before reasoned metaphysic, made in his own image all things in nature through not understanding.² This activity of mind is more creative than that which comes later, when man by understanding explains his own mind and comprehends things.

All early stories were held to be true. Tropes, and in particular metaphors, supposed to be ingenious inventions of authors, were necessary modes of expression for the first poetic nations. No facile abstractions being ready, a vehement effort was necessary to utter what was meant. Poetic diction arose precisely through poverty of language and need of explanation.³ It resulted from a kind of strength in weakness.

¹ i., 214.² i., 251-252.³ ii., 305.

In the early religions there was something that may be called a philosophy; but this philosophy existed as poetic thought, not as recondite wisdom formulated in abstract terms, which the men of the first ages did not possess even when they were founders and legislators.¹

The insight into the minds of early men thus summed up caused Vico flatly to reject the view universal among the learned of his time, and only with difficulty expelled by discoveries made long after, that a deep philosophy lay hidden beneath the Egyptian hieroglyphics. The opinion, he says, must be wholly uprooted that "hieroglyphics were invented by philosophers to conceal therein the mysteries of deep abstruse wisdom".² Hieroglyphic writing, in which pictorial representations are used, is characteristic of the early age of man, and could contain nothing but the "poetic wisdom" of religious myth and the "vulgar wisdom," or common sense put into order, of practical legislators.

The reputation of the theological poets for wisdom has come largely from the philosophers, who found it convenient to take up expressions from them in setting forth their own meditated doctrines, which they were thus able to support by the authority of traditional religion. What the poets had felt touching common wisdom, that the philosophers understood in the way of recondite wisdom; so that the poets may be called the sense, and the philosophers the intellect, of the human race. The beginning is always from sense.

As we shall see in more detail later, Vico, in dismissing the notion that early thought was philosophical in the proper meaning of the term, does not in the least intend to depreciate it. On the contrary, what he is apt to depreciate is too much method and system, which he finds repressive of genius. And it is in a classification of the ages furnished by what he knew of the Egyptian annals drawn from Manetho that he finds a basis for his own "eternal ideal history." To this we must now pass on.

Egyptian history was divided by the annalists into the ages of gods, heroes and men. Taking over this division, Vico proceeds to divide the history of every people, so far as it is

¹ See i., 191, where Vico incidentally contrasts "*la sapienza volgare di tutti i legislatori e la sapienza riposta degli più riputati filosofi*". In principle, Vico holds, the legislators and the Platonic philosophers agree in the belief in a divine providence and in the immortality of the soul; but the legislators drew on the primary and "poetic" wisdom, while the procedure of the philosophers was secondary and reflective, not creative. Belief in immortality was much older than Plato, who did not first diffuse it among the peoples, but took it over from them.

² i., 278.

determined from within, into the "divine," the "heroic" and the "human" ages. These, unless extraneous causes intervene, follow one another of necessity in this order; though for different peoples the corresponding ages are not necessarily simultaneous. The Greeks, for example, made the transitions in advance of the Romans. The first age, the "age of the gods," he sometimes calls "theocracy"; but, in dealing with it in detail, he is much more preoccupied with cult and myth than with mode of government, which begins to interest him distinctively only in the second or "heroic" age. In fact, he has no conception of the difference between government through a priestly caste, as in the East, and the mere permeation of life by religious sanctions wielded by heads of households and by civil magistrates, as among the Greeks and Romans. For him, so far as religion is concerned, the second age continues the first, except that he tacitly supposes the creativeness of phantasy to have sunk, and the rising social interest to be in political government. Vico's "heroic age" is essentially military aristocracy, in which those rule who are held to be more allied in race to the gods. From this political form, a government by men of forceful action, the transition is to democracy, or recognition of equal legal rights on the ground of a common humanity; but democracy is only the first form of the "human" age. Generalising from the case of Rome, Vico conceives the democratic republic as culminating in a monarchy. By the imperial monarchy the law of equal justice was continually extended; and this kind of monarchy he finds to have been restored, after the collapse of the empire and the long reign of the "returned barbarism" in Europe, by the "enlightened despotisms" (as they have since been called) of the Continent in his own time. The culminating monarchy of this type is to be completely distinguished from the early "heroic" monarchies, in which the king was simply the chief of a "severely aristocratic republic." A remains of that order he finds, among contemporary nations, in England and in Poland, which he classes together because of their surviving element of political aristocracy. Examples of modern democracies are the "free cities" included in the federal systems of Holland, Switzerland and Germany.¹

The ages of gods, heroes and men have different characteristic modes of figured expression. The language of the first is hieroglyphic or sacred or divine; of the second, symbolic or by signs or heroic emblems (*e.g.*, on shields); of the third, epistolary, to communicate at a distance the ordinary needs

¹ iii, 1028-1029.

of life. On spoken language, Vico adopts the view of those ancient thinkers who held that in the first languages there must have been some resemblance between word or sign and thing signified. From this, he observes, it does not follow that there must have been one universal language; for, while the utilities and necessities of human life are the same everywhere, the different peoples have differences, as well as communities, of nature which express themselves in languages as in customs. Conventional language, in which the sign has become simply a sign, belongs to the third period. Hieroglyphic signs, in which external things are figured, in whatever people they appear, come first, and belong to the stage of poetic or mythologic thought. For Homer, in the heroic age, the "language of the gods" means a language more ancient than his own. Vico allows that in all this there is something of abstraction. In concrete reality, one kind of age and one mode of language does not wholly displace another. The germs of all coexist from the beginning; for those who fabricated the gods and believed their own nature to have in it a mixture of the divine were, after all, men.

In Vico's theory of political changes, the Romans, as all who have dealt with his work are careful to impress on their readers, were his model. Yet, though he early gave up the direct study of Greek, he knew well the difference between Roman and Greek civilisation, which he always treats as more refined and more humane; and he has subtle remarks on the different modes of development within the general likeness presupposed by his doctrine. The Greeks, he says, through their more rapid passage to a refined civilisation, retained, side by side with this, the myths, divine and heroic, of their time of barbarism, while the slower-moving Romans forgot theirs. The French, among modern nations, he finds to be most like the Greeks. Thus, in the transition from the returned barbarism that followed the break-up of the Roman Empire, poems of Homeric kind could be written among them while the subtlest scholasticism was going on. In the simultaneity, in the twelfth century, of poetic literature drawn directly from mythical and legendary sources with the pursuit of philosophy in the school of Paris, they restored the Atticism of the Greeks.¹

POETIC CHARACTERS.

Such continuance of an older age in a newer supplies Vico with some of the hints for his profoundly original theory of

¹ i., 124 (and elsewhere).

"poetic characters". He was not, of course, the first to maintain that some personages supposed to have been historical were really mythical. Aristotle, as quoted by Cicero, denied that Orpheus was historical; and Vico knew this denial. No one, however, before him had done anything to generalise the notion of such a quasi-historical but really "poetical" or mythical character; and even those who in the nineteenth and twentieth centuries have adopted similar views in particular cases have perhaps never generalised their explanation as Vico did. For Vico thinks of his explanation not as applicable to any age whatever without definition of its place in the series of times, but distinctively to ages or nations on the borders of civilisation and barbarism, in which vivid fancy is still socially creative. By "poetic characters" he means personages who, with or without the nucleus of some "particular man in nature," were imagined in the late, rather than the early, myth-making age as representatives of legislative or political systems or as symbolising phases of culture. Of these are the Hermes Trismegistus of the Egyptians, the Orpheus and Dædalus of the Greeks, the Zoroaster of the Persians, all of whom became for the learned of after-times, as he puts it, particular men replete with deepest abstruse wisdom. Being the effective discoverer of this as a generalised mode of explanation, Vico naturally applies it too widely. Compared with what has been made out since his time, his knowledge of the relevant facts was slight; and, even if he had always borne it in mind, he could have made little use then of the precaution dictated by logical method, that each case must be submitted to a separate empirical investigation. Figures that seemed to him much alike turn out, when closely examined, to have quite different degrees of historical reality. Pythagoras, for example, might in Vico's age seem classifiable with Orpheus as simply the ideal representative of a movement. Yet for us, though no authentic record is preserved of anything written or spoken by him, he has become increasingly a tangible historical figure; while Orpheus remains wholly mythical. Again, Solon is a known personage in Athenian history and the author of verses which remain, while Lycurgus as actual legislator of Sparta is at least very doubtful. In qualification, however, of this criticism, it must be said that Vico is rarely dogmatic in denying every trace of "historicity"; though he is quite peremptory in setting against the prejudices of "the learned" the complete predominance, in the most typical cases, of the imaginary over the historical features. And in detail he often (with or without precursors) exactly

hits the mark. Special scholars agree that the Zoroastrian oracles, the *Pœmander* of Hermes Trismegistus, the Orphic verses, and the *Golden Verses* ascribed to Pythagoras, are all compositions of which the ascription was false. In the Greek writings ascribed to Zoroaster, Vico, like Porphyry, finds a doctrine that was new vended as old. The Zoroastrian oracles, in common with the Orphic verses, he says, savour too much of Platonic and Pythagorean philosophy.¹ The name itself of Zoroaster he takes to be a generalised name for founders of peoples in the East, as Hercules was a name for their leaders in the West.² The earliest form of Zoroaster, he concludes, was the Bactrian; as the earliest form of Orpheus was the Thracian. The later Orpheus, who brings the wild beasts of Greece to humanity, is "a vast den of a thousand monsters."³ Discussing what is related of the life of Pythagoras, he makes it the occasion of an argument against the supposed extensive travels of the Greek sages and the carrying over of detailed traditions from one people to another; and especially against the asserted derivation of the fables of other nations from corruption of the sacred stories of the Hebrews. He admits likeness, however, and the impetus of his own thought occasionally carries him on to comparisons which, if sustained, would have removed the boundary that he had set for himself and brought him, on another line, into more dangerous collision with consecrated positions.

His celebrated "discovery of the true Homer" is set forth at a later stage of his work, after he has dealt more circumstantially with the characteristics of poetic thought. To that stage we must leave the question in what sense Vico held that the "prince and father of all poets" was himself a "poetic character" and how far his theory or that of his successors is tenable.

¹ ii., 684.

² i., 72.

³ i., 85: "un vasto covile di mille mostri".

(To be continued.)

V.—CRITICAL NOTICES.

The Mind and its Place in Nature. By C. D. BROAD, M.A., Litt. D., Fellow and Lecturer in the Moral Sciences, Trinity College, Cambridge; Author of *Perception, Physics, and Reality* and *Scientific Thought*. (Tanner lectures delivered in Trinity College Cambridge, 1923.) Kegan Paul, 1925. 16s. Pp. x, 674.

THIS important book carries on the work of Dr. Broad's two previous books; the three together give the system of his philosophy of mind and matter. The level of those books is well maintained, and (to my mind) definitely surpassed in one chapter (the last).

The book begins, after an introduction, with two chapters discussing the problem from the level of enlightened common sense. The introduction considers various meanings of the words "Monism" and "Pluralism," and decides in favour of a plurality of material substances, leaving the question of mental substances open. Chapter II., on "Mechanism and its Alternatives," begins by a definition of "Pure mechanism," which is held to involve (a) a single kind of stuff, (b) no changes except changes of position, (c) a single elementary causal law, (d) a single and simple principle of composition of mutual influences of particles. I think this definition is a little too reminiscent of the eighteenth century; it might have suited Lamettrie or Laplace, but seems somewhat too narrow for modern physics. I am not sure whether Dr. Broad would consider that electrons and protons are the same kind of stuff, or two different kinds. But I am convinced that mechanism does not demand the reduction of the law of gravitation and the laws of electromagnetism to a single law. Moreover, as is evident from Weyl's work, the question what is one law and what is several is more or less arbitrary: a little ingenuity often makes it possible to express by one formula what was previously expressed by several. The question is perhaps not very important, since hardly any one now-a-days supposes that a mechanical explanation of everything is possible in any sense at all analogous to Dr. Broad's. There is, however, a quite different sense which, true or false, is still important. It may be that matter forms a mechanical system, and that the events not dealt with by physics are correlated with those that are in such a way that they can, at least theoretically, be inferred from physical events when the laws of correlation are

known. A man who held this view would probably say that he held a mechanistic theory.

Dr. Broad discusses next two forms of vitalism, called "substantial" and "emergent" respectively. The former, exemplified by Driesch, holds that in living bodies there is a substantial component, the "entelechy," which is absent in other matter; this view is rejected. The latter holds that vital characteristics are exhibited by matter having a suitable structure, and having no peculiar component, but that these characteristics cannot be inferred from the characteristics and structure of the parts. The word "emergent" plays an important part in Dr. Broad's book. A whole has an "emergent" characteristic when "the characteristic behaviour of the whole *could not*, even in theory, be deduced from the most complete knowledge of the behaviour of its components, taken separately or in other combinations, and of their proportions and arrangements in this whole" (p. 59). He continues: "I cannot give a conclusive example of it, since it is a matter of controversy whether it actually applies to anything. But there is no doubt, as I hope to show, that it is a logically possible view with a good deal in its favour." This may be admitted, and undoubtedly the possibility is one which it is important to bear in mind. But I think it is going too far to suggest that chemical compounds exhibit emergent characteristics; it seems far more probable that, if we had greater mathematical skill, we could calculate their properties from their structure. Perhaps, however, Dr. Broad does not really mean to deny this.

The view that all the properties of a living body can be explained from its structure and the properties of its parts is called by Dr. Broad "biological mechanism." He has an argument, which I am quite unable to accept, to show that this view involves Deism. He argues that machines, so far as our experience goes, never come into existence without design: a machine may be made by another machine, but in following this process backwards we come always to someone who designed a machine. Therefore, if living bodies are machines, they must originally have been made with design. Once more we are struck by the eighteenth century flavour, and reminded of the watch and the Watchmaker. Two answers at once occur to one. First, we do not know that life had an origin; it may have been carried by spores from one region of the universe to another, and have existed throughout all time. This, however, is not the fundamental answer. The fundamental answer is that, if biological mechanism is true, living organisms would have emerged if chance had brought the right components together in the right structure. If it be said that this is very improbable, the answer is that only an infinitesimal proportion of the matter known to science is alive; one would therefore argue from the known facts that the circumstances leading to life must be exceedingly improbable, since they occur so rarely. The question is of some importance in Dr. Broad's theory, since the desire to avoid the argument

for Deism is his main motive for preferring emergent vitalism to biological mechanism (see pp. 93-94).

Chapter III., on "The traditional problem of body and mind," discusses the arguments, both philosophical and scientific, against reciprocal interaction of body and mind, and decides that they are invalid; it also decides that there are no valid arguments for or against complete psychophysical parallelism—or psychoneural parallelism, as Dr. Broad prefers to call it. While entirely agreeing with both these conclusions, I cannot but feel that Dr. Broad's decision, at this stage, to confine himself to the level of enlightened common sense makes his argument provisional and indecisive. His conclusions could only become definitive if reached again, as I think they easily can be, at a more philosophical level.

Section B, on "The Mind's Knowledge of Existence," begins with a chapter (Chapter IV.) on "Sense-Perception and Matter." He gives the name "Perceptual Situations" to such as are naturally indicated by phrases like "I am seeing a chair" or "I am hearing a bell." These are distinguished from such situations as feeling cross or tired. "The important point about the perceptual situation is that we claim to be in cognitive contact with *something* other than ourselves and our states. This claim is just as obvious in those perceptual situations which are commonly believed to be delusive as in those which are commonly believed to be veridical.

. . . I will express the difference between the two kinds of situation by saying that the one does and the other does not have an 'epistemological object'" (p. 141). This is, of course, the orthodox view, but Dr. Broad seems not to have understood the position of those who question it. They would say that the "epistemological object" is constituted by beliefs, or by more primitive states of the sort that lead up to beliefs; and that the "ontological object" is that which makes these beliefs true, when they are true. The word "belief" is unduly definite. I wish to include such a situation as the following: you see what appears to be a heavy weight, you go to lift it up, and it turns out to be painted paper and quite light. To say that you "believed" it to be heavy may not be correct: you may have merely adjusted your muscles to the effort of lifting a heavy weight, and have had the accompanying sensations. You experience surprise, and say that you "thought" it was heavy, but this may be an intellectualising of what occurred. What probably occurred was that you acted as you would have done if you had believed and reflected, but that in fact you did not reach the point of explicit belief. This is the usual situation in every-day actions. When a cricketer catches a ball, he acts as he would have done if he had worked out its trajectory mathematically by a lightning calculation; but it would be wrong psychology to say that he "believes" it will pursue the course which it does pursue. Let us call such situations "pseudo-beliefs." They are characterised by action which a certain belief would render reasonable, and by the fact that this belief will arise if any relevant

belief arises. The beliefs in question, where perception is concerned, are generally, in part at least, expectations. Those who reject the usual analysis accepted by Dr. Broad would say that perception differs from sensation through the presence of beliefs or pseudo-beliefs concerned with correlates (present or slightly future) of the sense-datum. In this way, the whole problem of the "object" is shifted to the analysis of belief, where it is radically different, owing to the complexity of belief and its inclusion of elements not sensibly present. There may be good arguments against this view, but Dr. Broad does not give them, and does not seem aware that there is such a view. This question is, of course, connected with the question whether there is a valid distinction between sensations and sense-data.

Another point of considerable importance arises in this connexion. Dr. Broad says (p. 143): "It would be agreed, I think, that the epistemological object of any perceptual situation must be of the physical kind; and this simply means that, if there be an ontological object corresponding to it, it must be a physical object or event". Now the word "physical" is very ambiguous, and I have not found in this book any clear definition of it. The nearest approach to a definition is that the common-sense notion of a physical object involves four characteristics: (1) It is more permanent than the perceptual situation; (2) it is supposed to be public to a number of observers, and capable of being exhibited to different senses; (3) it is literally extended in space; (4) the objective constituents of the tactual and visual situations in which it is said to be perceived are held to be literally parts of its surface (p. 196). Of these, Dr. Broad regards the fourth as erroneous, but the other three may be held (I infer) to constitute his definition of a physical object (p. 204). I am not quite certain whether he uses "physical" and "material" as synonyms, but on the whole it would seem that he does. On page 633 he gives the following definition of "materiality": "Materiality is a complex characteristic. The fundamental factor involved in it is extension. This, if I am right, carries with it *some* extensible quality. The other characteristics are publicity, persistence, and existential independence of any observing mind." He says elsewhere that colours, for instance, do not depend upon the mind of the observer, but upon his body (p. 177); on this point I should hypothetically agree. But extension, publicity and persistence all raise problems which Dr. Broad does not seem to me to dispose of adequately. First: "space" is a very ambiguous term. There is the space of psychology, the space of physics, and the space of pure mathematics. The space of psychology and the space of physics are both instances of the space of pure mathematics, but otherwise they have not very much in common. By the space of psychology I mean the extension and spatial relations of sensa. A visual sensum is "out there" in this space, but in physical space it is "here". All my sensa are, for physics, inside my body; this is demanded by the continuity of causal processes, *e.g.*, light waves,

eye, optic nerve, brain, sensum. It is impossible to suppose that the sensum, which comes at the end of this process, is, for physics, in a place quite remote from that of its immediate physical antecedents. I think the confusion of visual space with the space of physics (generally unconscious) has been a potent source of error in this connexion. There is a sentence on page 201 which makes me think that Dr. Broad is not wholly guiltless in this respect. He says: "One cannot say, *in any literal sense*, that God's habits of volition, or a colony of unintelligent monads, or a group of inter-related sensa, have geometrical shape, size, or position" (my italics). I am sure I could persuade Dr. Broad that a colony of Leibniz's monads has shape, size, and position in exactly the same sense in which matter has these properties—meaning by "matter" that which satisfies the equations of physics. This must be evident, on reflection, to every one who has grasped the logicising of geometry.

As regards publicity and persistence, I should maintain that nothing public or persistent is ever experienced; sensa generate beliefs or pseudo-beliefs in public and persistent entities, and these beliefs can, with sufficient logical ingenuity, be interpreted in such a way as to be very likely true often though not always, but as held by common sense they contain elements which are almost certainly false. That is to say, perception differs from sensation by the addition of beliefs or pseudo-beliefs which are often mistaken—*e.g.* when a bird first sees itself in a looking-glass.

For these reasons, I should regard our knowledge of physical objects as far more questionable and inferential than it seems to Dr. Broad. If anyone argues that, in that case, we don't know much, I can only reply that this does not seem to me a *reductio ad absurdum*. If any one says that I have assumed the truth of physics in an argument which tends to show that physics is doubtful, I reply that this is a perfectly correct logical procedure; it would only be fallacious to assume a premiss if the conclusion were the *truth* of the premiss.

I pass by three chapters on Memory, Introspection, and Knowledge of other Minds, not because they are unimportant, but because the issues raised are to some extent separable from the central question of the status of mind. I will merely report that the last of these chapters considers a direct knowledge of other minds probable, on certain occasions, through telepathy, or "telegnosis" as Dr. Broad prefers to call it.

The next section, on "The Unconscious," shows considerable skill in avoiding the pitfalls of this topic, without ignoring the phenomena that have made it popular. Dr. Broad, on the whole, dislikes psycho-analysis and likes psychical research; these feelings have, I think, some influence on his views. There is a careful discussion of "mnemic causation," reaching a conclusion with which the present reviewer has no quarrel, though apparently he is expected to have. (Except, indeed, on one point, which concerns causation in general: Dr. Broad holds that causation is something more than

regular sequence, but he does not explicitly argue this question at all fully.) Having decided that mnemonic phenomena require the existence of "traces," Dr. Broad discusses whether these are to be mental or bodily, and decides that either theory is possible, unless there is evidence that minds can exist apart from bodies. This introduces Section D, on "Alleged Evidence for human survival of bodily death".

After rejecting the ethical arguments for immortality, Dr. Broad proceeds to consider the evidence accumulated by psychical research. It is maintained that there is good evidence for "possession," in which a medium's body appears to be controlled by a dead person (p. 395). The present reviewer knows too little about psychical research to venture an opinion as to the reliability of the observations to which Dr. Broad appeals. But the view put forward to explain them is one which is by no means easy to understand. It is suggested that an ordinary mind is a compound of two ingredients, one physical, an "emergent characteristic of a living body," the other called "the psychic factor," which apparently contains the "traces" upon which mnemonic phenomena depend, and is able, in certain circumstances, to unite with another body after its own body is dead. It is not suggested that the psychic factor is immortal, but only that it survives bodily death for a time—apparently a longer time in the case of a highly developed mind than where the mental level is low. When a medium is thus "possessed" by some one else's psychic factor, she exhibits the mnemonic characteristics of that other person. I find it difficult to see what advantages this new dualism has over the old dualism of mind and body. The only obvious gain is that, as we know nothing about the psychic factor, conjecture has free play. The psychic factor, we are told definitely, is not a mind, and may perhaps be physical; but that is nearly the whole of what Dr. Broad professes to know about it.

I come now to the most important chapter in the book, namely the last, on the "Status and Prospects of Mind in Nature". This chapter begins by enumerating seventeen possible theories as to the metaphysical status of mind and matter. For this purpose certain preliminary definitions are necessary. A "differentiating" attribute of a substance is one belonging to the substance in question, but not *necessarily* to all substances, and having also certain further properties: (a) it is "a determinable which is not itself a determinate under any higher determinable"; (b) if it belongs to any complex substance, it belongs to all its parts; (c) it is a single attribute, *i.e.*, not a conjunction or disjunction of attributes (pp. 22-23). (I think there are grave logical difficulties about (a) and (c), but we may let that pass). When an attribute applies to certain substances but not to others, it may happen that all the substances to which it applies are complex; in that case, it is non-differentiating, because of the failure of (b) above. When a complex substance has such an attribute, the attribute may or may not be theoretically deducible from the character and arrangement of the parts of the said

substance; in the latter case, the attribute is called "emergent". An attribute which is neither differentiating nor emergent is called "reducible," if there are substances to which it applies; if not, it is called "delusive". (Dr. Broad only calls an attribute delusive if it *seems* to apply to something without actually applying; but for the sake of logical exhaustiveness it would seem desirable to omit this condition.) Thus every attribute is differentiating, emergent, reducible, or delusive. This should give sixteen possibilities, according as mentality or materiality possessed one or other of the above four characters. Dr. Broad, however, proceeds slightly differently. His seventeen theories are as follows:—

1. Mentality and materiality are both differentiating attributes, and can both belong to the same substance.
 2. Same as 1, except that the two attributes are incompatible.
 3. Mentality differentiating, materiality delusive. Pure mentalism.
 4. Materiality differentiating, mentality delusive. Pure materialism.
 5. Mentality differentiating, materiality emergent. Emergent mentalism.
 6. Mentality differentiating, materiality reducible. Reductive mentalism.
 7. Materiality differentiating, mentality emergent. Emergent materialism.
 8. Materiality differentiating, mentality reducible. Reductive materialism.
 9. Mentality and materiality both emergent. Emergent neutralism.
 10. Both reducible. Reductive neutralism.
 11. Mentality emergent, materiality reducible.
 12. Mentality reducible, materiality emergent.
 13. Mentality emergent, materiality delusive.
 14. Mentality reducible, materiality delusive. 13 and 14 are called "Mentalistic neutralism".
 15. Mentality delusive, materiality emergent.
 16. Mentality delusive, materiality reducible. 15 and 16 are called "Materialistic neutralism".
 17. Both delusive. Pure neutralism.
- (The above numbering is mine.)

Dr. Broad begins by eliminating the four theories in which mentality is delusive; for this he relies upon what is essentially Descartes' *cogito*. He then proceeds to eliminate 6 and 8, of which the latter is identified with behaviourism, while the former is thought to have never been held. I should have said that Leibniz held it, though he was somewhat confused on the subject. The space in the perceptions of each monad was delusive, but the monads themselves formed a pattern according to their "points of view," and this pattern was geometrical. (See my *Philosophy of Leibniz*, p. 122.) This seems to me a quite respectable theory, and

I do not think that Dr. Broad adequately refutes it. I think that Dr. Broad, like Leibniz, has failed to realise how remote the space of physics is from the space of perception. The space of physics is represented, in Leibniz, by the pattern made of the monads, which God alone can perceive; this pattern is real, since the monads really have points of view which can be arranged in a three-dimensional order. My belief is that Leibniz could be re-written in terms of modern physics and mathematical logic, and that the result would be a system which could not be shown to be false.

Dr. Broad next rules out pure mentalism (number 3), because of his belief that there are physical objects. Here, again, I cannot quite follow him. There is one form of pure mentalism which seems to me not definitely refutable, and that is solipsism, in the sense that nothing exists except what forms part of the experience of the metaphysician in question. None of the inferences from what I experience to other existents seem to give logical certainty.

At this point, Dr. Broad introduces a discussion of what is meant by "mentality." Sentience, acquaintance, referential cognition are mentioned; also affective attitudes. On the basis of this discussion, he concludes that mentality is a complex attribute; he has already reached the same conclusion as regards materiality. This leads him to distinguish three kinds of neutralism: the neutral stuff may have (a) some material and no mental characters; (b) some mental and no material characters; (c) some material and some mental characters. He argues that each of these hypotheses is incompatible with the view that mentality is a reducible characteristic, and thus rules out 10, 12 and 14 of the above possibilities. By similar arguments he eliminates 11 and 13, if hypothesis (b) applies to the neutral stuff.

Dr. Broad discusses at some length Locke's view that matter might think, and decides that it cannot be refuted. On this ground he rejects 2, which is Descartes' view. Other forms of dualism are rejected on other grounds, which do not seem to me very cogent, although I agree with the conclusion. He argues that mentality cannot be a differentiating attribute, apparently on the ground that it is always found associated with organic bodies. This leads him to emergent materialism (number 7), as to which he says: "If there were no facts to be considered except the normal ones, and we rejected all the alleged abnormal facts dealt with by Psychical Research, I should regard Emergent Materialism as on the whole the most reasonable view to take of the status and relations of matter and mind in Nature. The only question would be whether one of the forms of neutralism might not be preferable" (p. 647). The forms of neutralism which he regards as possible are emergent neutralism (number 9) and one form of mentalistic neutralism (number 13). But on account of the phenomena studied by psychical research, which, he says, afford "some evidence for persistence after bodily death, but hardly any that justifies a belief in survival" (p. 651), he proceeds to suggest that "mentality is an

emergent characteristic of a *compound* composed of a living brain and nervous system and of something else which is capable of persisting for some time after the death of the body and of entering into temporary combination with the brain and nervous system of certain peculiarly constituted human beings called 'mediums'. This something else I call a 'Psychic Factor'."

This, however, makes less difference than might have been expected in regard to the problem of choosing among the seventeen alternatives enumerated above. Dr. Broad decides that the Psychic Factor may be itself a kind of matter, and that therefore, even after psychological research has been taken into account, emergent materialism remains the most probable view, while two forms of neutralism remain possible.

The enumeration of the seventeen theories seems to me very important in clarifying the issues, and at the same time exhibiting their complexity. Nevertheless, the book as a whole seems to me open to the criticism that it lavishes more care upon the superstructure than upon the foundations. I think Dr. Broad is too kind to matter, because he has not devoted enough care to the ambiguities of the word "space". For this reason chiefly, the discussion of physical objects in Chapter IV. seems to me to fail to prove its point. I feel a defect of the same kind in the fact that there is no adequate discussion of the terms "mental" and "material". The definitions of these terms are incidental and more or less perfunctory. As characterising mind Dr. Broad mentions sentience, acquaintance, referential cognition, and affective attitudes (p. 634 ff.). All of these require more analysis than he has bestowed upon them; or, if he holds them unanalysable, more proof that this is the right view. In regard to both mind and matter, Dr. Broad assumes that a certain degree of persistence is essential to substantiality. This view, it seems to me, is profoundly affected by the substitution of space-time for space and time, which leads to temporal in addition to spatial atomism. These are some of the grounds upon which I should prefer neutralism to either mentalism or materialism. They may be inadequate grounds, but they are not refuted by Dr. Broad. Especially the substitution of space-time for space and time seems to have failed almost unaccountably to influence his thought.

Dr. Broad's book is admirable in its detail, and in its sense of logical relevance. Any person who finds himself criticised cannot but profit by the attempt to answer his criticisms in detail, an attempt which the present reviewer hopes to undertake on another occasion. No philosopher who values accurate distinctions and careful reasoning can afford to neglect Dr. Broad's book.

BERTRAND RUSSELL.

Principles of Literary Criticism. By I. A. RICHARDS. London: Kegan Paul, 1925. Pp. 290. 10s. 6d.

THIS book holds the reader by the quality of its approach to literature; not by throwing any new light on dark places, but because it comes from a deep and genuine conviction that literature, as a branch of art, is something momentous, is part of the stuff and meaning of life, is more than a pastime to be analysed by connoisseurs and exploited by hedonists. All who feel about art in this way will welcome the book warmly, but not all such readers will welcome it without serious reservations. Apart from the immature, there are two principal kinds of such readers: those who think that their feeling about art finds its justification in some mystical view of the scheme of things, and those who can only find satisfaction in some rational justification. The former will be irritated by the bricks that Mr. Richards heaves at them. The latter will begin by being irritated and will end by being puzzled. Why should he think that no one who does not accept his theory of aesthetics has a right to the belief that the place of art in life is a "supreme" one, when it seems so obvious that, apart from exciting our sympathies, he says nothing that can confirm that belief in any way?

His position may be stated as follows. Whatever the field of study, no explanation of the phenomena can be tolerated that is not scientific through and through, and, whatever the field, this condition can only be fulfilled by substituting things that can be measured for things that cannot be measured. If aesthetics is our field, the first thing we require is a definition of value, and we shall accordingly make the following substitutions. In place of a thought we put the external physical stimulus that causes the organism to respond. In place of a feeling we put the particular configuration of impulses (largely unconscious) occurring in the nervous system and brain on application of the stimulus. In place of a desire, volition or conation we substitute the active adaptation of the organism to environment. Thus all mental events will be completely expressed in terms of external stimulus and neural process. We have completely eliminated the dualism of subject and object (*e.g.*, a thought is no longer a thing of a particular kind referring to another thing of a different kind), and this was necessary because it is this dualism that makes any field in which it occurs refractory to science. A completely scientific psychology would reduce every whole which seems to be a complex compounded, through the subject-object relation, of mental and physical elements to a homogeneous causal chain (*e.g.* from black marks on paper to the tragic *catharsis*) of the kind just indicated. From this point of view, to study aesthetics is merely to study a part of the psychological field, the part namely to which the notion of value is relevant. Therefore all we require is a definition of value, which results from the above three steps as

follows. The neural part of the process indicated may be more or less completely organised, and we shall call experiences valuable in proportion as the organisation is more or less complete, *i.e.*, in proportion as it permits the least thwarting of the most important impulses. Mr. Richards discusses many of the puzzles that have exercised art-critics and urges that in each case the solution can only lie in applying this definition. He admits indeed that he cannot explain everything. For instance, it remains 'mysterious' and 'unpredictable' why the response to several stimuli (and we are always concerned with more than one simple stimulus) should be more than a summation of their effects. But this, he says, is only because neurology has not made progress enough; a complete explanation in neural terms is theoretically possible, and no other method can give an explanation at all.

Apart from minor objections (for instance, a doubt whether in the definition of value what is to be defined is not already involved in the phrase 'important impulse') there are two connected objections that seem fatal to this position as a whole. These are, first, that the reduction of all the phenomena in the particular field studied by Mr. Richards to forms of neural process is in fact incapable of organising the field scientifically; and, secondly, that, if the reduction were scientific in the sense that he supposes it to be, what would be explained would not be what his theory is constructed to explain.

First, then, it is clear that all that Mr. Richards' substitution-method can at best make possible is an empirical induction to the effect that certain events under certain conditions have certain results. Thanks to his substitutions, it might conceivably be easier than it would otherwise be to establish, by enumeration and classification of instances, a reasonable expectation that certain things will happen if certain other things happen. Now the results which the physicist attains by eliminating everything except quantities that can be measured are essentially different from this. The problem for physics is to reduce to a minimum the indefinable assumptions required for an explanation of the phenomena such that all the phenomena can be derived from the assumptions and a necessary connexion between them all can be mathematically demonstrated. Clearly nothing in any way resembling this element of necessity is or could be introduced into the field of aesthetics by Mr. Richards' substitutions. But the explanations offered by the physicist satisfy in proportion as they are pervaded by this element; their satisfaction lies in their being a closed system of inferences. Hence, if Mr. Richards ascribes to his theory, as he certainly seems to do, the same kind of explanatory merit as that possessed by modern exact science, he must be mistaken.

But, secondly, if he were not mistaken on this point, he would still be no nearer his goal; if the field could be scientifically organised by his substitutions, what would be explained (if anything) would not be what is required. For what is required is to justify the belief that art

should occupy a supreme place in life. Now if this justification is to be effected by explaining the phenomena given in the æsthetic field, it seems clear that the explanation must be one that elucidates the intrinsic nature of the phenomena. But no scientific organisation of the field by a substitution-method can yield an explanation of this kind, since the vital principle of that method is to explain the phenomena more and more completely by saying less and less about their intrinsic nature. When the physicist observes the readings on measuring instruments, makes them consistent with one another and demonstrates the mathematical relations between them, his work is a satisfactory explanation of the phenomena—the deliverances of the senses—only because he pays a price; it is less rigorous, less satisfactory as an explanation, in so far as any part of the price is withheld and expressions which seem to elucidate the intrinsic nature of the phenomena remain unreduced. In other words, the purpose of Mr. Richards' application of the substitution-method is based on a mistake as to what it is that a scientific explanation explains; there is no point in it unless it elucidates the intrinsic nature of the phenomena, and this it cannot do. He might demur that his purpose is *not* to explain the intrinsic nature of the phenomena: since, he might say, the essence of his position is that any explanation must be scientific, then, if a scientific explanation cannot bear on the intrinsic nature of the phenomena, that only shows that we misconceive the problem if we think that the question as to the supreme place of art in the scheme of things involves any question as to the intrinsic nature of the æsthetic experience. But (apart from the fact that, as we have seen, his method cannot give the scientific results desired) it seems clear that this reply rests on another mistake—namely the assimilation of his field to that of the physicist. If his field is similar in kind to that of the physicist, then indeed none of the problems arising in it could be problems as to the intrinsic nature of anything; for the physicist defines his field expressly to exclude such problems. But in fact the two fields are different in kind. Mr. Richards' field is formally comparable not to that of the physicist but to the universe considered as a whole. It shares with the universe a property that may be called phenomenal heterogeneity: that is to say, both the universe and Mr. Richards' field *seem*, unlike that of physics, to be compounded of two substances differing in intrinsic nature, namely consciousness and physical events. Mr. Richards thinks that this difference has only to be reduced by definition for his field to become completely amenable to scientific treatment. Were the universe as a whole in question, this would clearly be to put the cart before the horse; it would not be scientific to attempt an explanation of the universe by removing the difference unless it could first be shown that the difference between consciousness and the other phenomena is not an ultimate difference of intrinsic nature. And the same holds in Mr. Richards' field. The field of the physicist is abstracted from

the universe by definition; but that of Mr. Richards is a sample taken from its living substance, piping hot as it were.

The difficulty, in short, is to see why it should ever have occurred to Mr. Richards to make a sacrifice—the exclusion of consciousness—which, in his field, cannot possibly bring him the recompense which it brings to the physicist in *his* field, and which, even if it could, would be nothing to the purpose of aesthetics as he himself conceives that purpose. Whatever the reason, it results in spoiling the otherwise admirable temper of his book. It prevents him from doing justice to those with whom he disagrees. But for his determination to exclude consciousness at all costs, he would see that both ‘revelation’ theories of art (according to which the æsthetic experience involves, in some sense, knowledge of ultimate reality) and Professor G. E. Moore’s theory (according to which intrinsic value is an unanalysable quality) have the merit of being highly relevant to the problem. If he had seen that theories of both those types are right in principle, since they both bear directly on the intrinsic nature of the æsthetic experience, he might have wondered whether his own view is right in principle; and, if so, he might have improved his book by approaching these rival theories with more respect and with less flimsy rebuttals.

S. P. WATERLOW.

History of Indian Logic (Ancient, Mediæval, and Modern Schools).

By SATIS CHANDRA VIDYABHUSANA. Pp. xliii + 648. Published by the University of Calcutta, 1921.

MAX MÜLLER, writing in 1899, said that papers published even as long ago as 1824 could still be recommended to those wanting trustworthy information on Indian logic and philosophy; the reason being that Indian philosophy says exactly what it means, and is therefore easy to report. His reference is to Colebrooke’s papers on the Philosophy of the Hindus, which seem to have been the earliest attempt to give a sketch of the six systems to European readers. Barthélemy St. Hilaire wrote a *Mémoire sur la Philosophie Sanscrite, le Nyāya*, in 1839; and Max Müller himself contributed an appendix on Indian Logic to Archbishop Thomson’s *Laws of Thought*. Certainly these early sketches are trustworthy: but they are not philosophically speaking intelligible, just because they are outlines, and can only tell us what the Indian philosopher said, and not why he said it and what he meant by it. We are only now beginning to understand what they meant: and so far as the Nyāya or logical system is concerned we owe the possibility of understanding principally to the work of two or three English-speaking Indian Sanskritists, of whom the late Satis Chandra Vidyabhusana was one. This book is his monument, embodying the results of his life’s work, and was published soon after his

death. He learned Tibetan, because he knew that important Buddhist logical treatises of which the Sanskrit originals are no longer extant in India were preserved in the great Tibetan collection of translations called the Tanjur; and himself travelled beyond the northern frontiers of India in search of manuscripts. The account of Buddhist (and Jaina) logic which he was thus enabled to publish in 1910 in his *Indian Logic*, Mediaeval School (which is incorporated in a revised form in the present work) formed an invaluable contribution to our knowledge of the history of Indian logic. The rest of the book includes work that was published at various times and covers the whole development of Indian logic from the earliest writers down to the nineteenth century. For the Nyāya has had a history of some two thousand years and is still a living school, continuous with though very different from the school as it existed from about the commencement of the Christian era. It is almost as if the Peripatetics were still teaching the *Organon* in Athens.

Vidyabhusana's contribution is principally to the history of logical doctrine. But contemporaneously with his earlier activities, and before, the important work of editing and thus making accessible for the first time the fundamental texts of this and allied schools had been going on: and in the years 1910-1918 the two oldest commentaries on the brief aphorisms which constitute the earliest document of the Nyāya school were translated by another Indian scholar, Dr. Ganganatha Jha. In the light of these basic texts and of this pioneer translation, and with the assistance of Vidyabhusana's researches into the history of the system, we are now able to arrive at something like an understanding of what the Indian logician *meant*, instead of having to rest content with sketchy reports of what he *said*. A few contemporary scholars scattered about the different countries of Europe—Jacobi, Stecherbatsky, Faddegon, Poussin, Strauss, Suali, Keith,¹—have already published works dealing with or relevant to Indian logic which begin to make it inexcusable that we should continue to ignore the very remarkable achievement of India in logic,—an achievement comparable with,

¹ Dasgupta's work on the *History of Indian Philosophy* was published in Cambridge (Univ. Press, 1922) and therefore should perhaps be included in this enumeration, although the writer is of course an Indian.

Earlier scholars like Ballantyne, Cowell, and Gough, did invaluable work, though the basic Nyāya commentaries were then inaccessible. Cowell however saw them in manuscript, and this enabled him to settle their relative chronology, and to draw attention to their importance. Another European Sanskritist whose work was in some sense more important to the understanding of Indian logic than that done by any contemporary was the late Arthur Venis, once Boden Scholar of Oxford University, and the last of the European principals of the Sanskrit College at Benares. For it was he who diverted the learning of famous pandits of that institution to the task of editing the ancient Nyāya texts. Similar work was carried on for half a century in the institution's journal, the *Pandit*, under the superintendence of his predecessors (Ballantyne, Gough, Thibaut) and himself.

closely parallel to, and quite independent of, the Aristotelian organon. Vidyabhusana indeed—with what is certainly a mistaken generosity—argues that the formulation of the Indian syllogism is derived from Greek logic. But there is no evidence at all for this, the supposed correspondences which he adduces being for the most part not correspondences at all, and, where they are correspondences, being no more than inevitable parallelisms of thought. Another matter in which Vidyabhusana's judgment is unreliable is in regard to the antiquity of the origins of his system, which he traces back to legendary times and names, although in fact there is no evidence of its existence before the Christian era, and Buddhist literature which belongs to the third and second centuries B.C. and which is full of controversy presented sometimes in a stereotyped debating form but still quite innocent of logic, provides strong negative evidence that real logic as known to the Nyāya *could* not have existed then. But apart from such occasional lapses Vidyabhusana was a historian careful of truth and with an infinite zest and capacity for the accumulation of information about logical writers and their doctrines. The result is a book which is a kind of biographical dictionary of writers on logic and catalogue of their tenets.

I have been tempted to deal with the bibliography of the subject by a suspicion that those who devote themselves to it are rather in the position of the Seriphian who was so magnificently snubbed by Themistocles. For the subject is as obscure as Seriphus; though I think it deserves a better fate. In a recent article in *MIND* (vol. XXXIII, N.S. no. 132) an attempt was made to indicate the character of the earlier formulation of the Indian syllogism and of the canons of syllogism which developed out of it. This dealt merely with the formalism of Indian logic, and only with one aspect of that, no attempt being made to follow this into the syllogistic and theory of fallacies with which it connects itself. Nor was any reference made to the elaborate *Sophistici Elenchi* which forms the closing chapter of the Indian, as of the Aristotelian, organon, and which first treats of what are known to Aristotelian logic as fallacies *in dictione* and then exposes the various sophistical devices with which inference as such may be attacked,—such as the dilemma that the middle and the major are either given as united or as independent units: if the former, there is nothing left to prove; and if the latter, proof is *ipso facto* impossible. Again, inference ultimately rests on evidence (the 'example' of the Indian syllogism) which is the *final* means of knowing that M carries P with it: but the sophist proceeds to demand *proof* that the evidence proves what it claims to prove. The answer to this is given by the oldest commentator in a piece of question-and-answer dialectic which might have come from a Platonic dialogue. "What persons take a light, and for what purpose?" "Persons who want to see, for the purpose of seeing what can be seen." "Then why do not people who want to see the light take another light to see it by?" "Because a light is seen

without another light to see it by." "Well, for what purpose is the example adduced in a syllogism?"—The conclusion is as obvious as it is conclusive, and the citation need not be carried further. But it is not in these more formal parts of the system that the real calibre of the Indian logician makes itself most effectively felt, but rather in the difficult polemical passages when he comes to grips with the Buddhist sceptic who denies (or, to speak more accurately, does not admit) the reality not only of external things but of thought itself, and who sometimes anticipates with almost verbal correspondence the arguments against the reality of causation (for instance) which the late Mr. Bradley employed in his *Appearance and Reality*. From the first, Indian logic—apart from its invention of a syllogistic art which need not fear comparison with Western syllogistic—raised fundamental problems and sought fundamental solutions. These problems are in the main metaphysical or epistemological. But in the seventh century commentator—known as Uddyotakara or the Illuminator—problems of a purely logical nature appear to be emphasised, and it may be that his work is the greatest purely logical treatise that India produced. His most characteristic logical doctrines however—such as his rejection of the abstract universal premise 'M is P' as the nerve of inference, and his apparent insistence that the inference is from the concrete SM and not from the abstract M—were ignored by the later school, who adopted the more easily intelligible and workable view, taught by Vaiśeṣika and Buddhist logic, that we argue from a *vyāpti* or inseparable connexion of the abstract characters M and P. He seems to be trying to give expression to a view of universal connexion as having its ground in the nature of the individual. But it is not easy to see what his *positive* position was: all that is certain is that he definitely *rejected* the notion of an inseparable connexion of characters in the abstract. The rather elusive nature of his logical doctrine probably explains the fact that two centuries after his death the next great commentator (Vācaspati Miśra, c. 840) speaks of his teachings as antiquated and requiring to be re-juvenated by a re-interpretation. Enough has perhaps been said to establish a *prima facie* case for Indian logic, and to indicate that the disinterested labours of scholars like Vidyabhusana may some day prove to have been worth while.

H. N. RANDLE.

Wertphilosophie. By WILLIAM STERN. Leipzig: Johann Ambrosius Barth, 1924. Pp. 474.

It is only within the last few years that William Stern has begun to attract attention as a philosophical thinker. His philosophy of Critical Personalism is expounded in a work entitled *Person und Sache*. The first volume, *Ableitung und Grundlehre*, published in 1906, has only recently appeared in a second edition; the next

volume, *Die menschliche Persönlichkeit*, published in 1918, has already reached a third edition. The third and concluding volume, *Wertphilosophie*, with which we are here concerned, was published in 1924.

Stern's Critical Personalism has been developed chiefly in opposition to what he calls Abstract Idealism, though it shares with it the conviction that the "true nature of being is not to be found on the level of the given". Nevertheless it regards the real not as idea, abstract and with universal validity, but as a hierarchy of persons, concrete and individual. Critical Personalism is also opposed to recent forms of philosophical humanism and pluralisms of the pragmatist type, rejecting their anthropomorphic features and striving for a view of the Whole as *unitas multiplex*.

Critical Personalism is essentially metaphysical resting on ultimate convictions not of the nature of propositions appealing simply to the reason. Metaphysics itself cannot be conceived as something final and fixed: it is always in flux, conscious that its task can never be fully accomplished. Though the existence of facts and the validity of values are categories of quite different order metaphysics unites them in a form of ultimate conviction. In this volume Stern seems to set himself two tasks: to describe conceptions in which fact and value are related; and to give an analysis of the realm of values. He endeavours to expound a metaphysics in which *Lebensanschauung* and *Weltanschauung*—philosophy of life and view of the world—are united.

"Person" is by far Stern's most important conception, and to avoid misunderstanding in the later use of that term and related expressions, such as "I," it is best to indicate at once what he intends by it. "By person is meant an existent, which, notwithstanding the multiplicity of its parts, forms a real unity of its own kind and worth, and as such, in spite of the variety of functions of the parts, achieves a unified purposive activity of its own." "A thing is an existent, which, formed of a number of parts, constitutes no real unity of its own kind and worth, and with a variety of functions has no unified purpose of its own." Some of the English words used here, as for example, purposive, are not to be taken in a very definite or specific sense. The concept of person is very different from that of consciousness: it is much wider in its extension.

Turning to the subject of value Stern maintains that the search for an *a priori* rational knowledge of value fails. Whether conceptual or presentative, it would be simply cognitive and not have the practical character of value as realised in the person. On the other hand mere empirical knowledge is insufficient, for the norm or standard lies outside of the empirical. Stern criticises the attempt to arrive at a philosophy of values through consideration of the particular values found in human culture. Philosophies of value so oriented in our day satisfy themselves with a number of ultimate ideas of values under which the individual

spheres of culture are subsumed, as the intellectual, the æsthetic, the moral, and the religious. They fail to show how these values are related to one another, to a philosophy of life, or to a co-ordinated view of the universe. Further, the principle of value is infinitely wider than human culture and its different spheres. The metaphysic of value must therefore be something other than a philosophy of culture. It cannot remain bound up with such an anthropomorphic view, but must start from categories which, not dependent on the limits of human culture, govern the whole world of values. The theory of values of Abstract Idealism must also be rejected, for the notion of an abstract existence of ideas of value is an absurdity (*Ungedanke*). It is a self-evident conviction that true being can be conceived only as effective being. As a matter of fact, Abstract Idealism has always tended to hypostatise and personify its ideas. But the significance of ideas is derivative. In a footnote Stern makes the important remark that the derivative character of ideas is not in their psychological existence in the consciousnesses of individual men. They have an objective character as constituents of a higher whole, and are in a manner normative for the individuals co-ordinated within that. They are derivative in that they have no existence outside a "person" in Stern's sense.

The ultimate conviction from which the consideration of values starts may be expressed in the affirmation: "I value, therefore I am." (*Ich werthe, also bin ich.*) Stern then passes to what appears a highly debatable inference, in fact, a *non sequitur*, for he says that the capacity of valuation implies the intrinsic value of the person valuing. "I value, therefore I am—value." (*Ich werthe, also bin ich—Wert.*) For in valuing the "I posits itself as the value centre of a value cosmos". The person affirms value as having validity, but as validity can have no meaning unless value exists, it posits it as existing. Thus we are supposed to have arrived at the recognition on the one hand of the value of the "I," in valuing, and on the other of the value or values of the "not-I", the objective, as valued. "I have value," and "There are values". There is no definition of the concept of value, since there is no more elementary concept to which it might be referred. So much, however, may be said, that value is an adjectival conception: it is not itself a substance but attaches to such. We cannot rightly say: "This is a value," but only "This has value, or, is valuable".

Stern classifies values as *Selbstwerte*, *Strahlwerte*, and *Dienstwerte*. *Strahlwerte* might be translated literally as "radial values". For a number of reasons it seems advisable to use the following equivalents for those terms: Intrinsic, Constituent, and Instrumental Values. Stern maintains that error is frequently incurred, both in treating Constituent Values as Intrinsic, as in the Abstract Idealist hypostatizing of the ideas of the good, the true, and the beautiful, and also in regarding them as merely Instrumental, as by Pragmatism.

This distinction of a class of values intermediate between Intrinsic and Instrumental Values is one of the original and most interesting contributions of the volume.

Intrinsic value is absolute: but this "absoluteness" must not be identified with universality, immutability, eternity, or timelessness. Intrinsic value is absolute in the sense that it depends on nothing outside of it. Following out the implications of his fundamental metaphysical conviction, Stern equates intrinsic value with person. A person is an existent whole having intrinsic value. Only what is a real whole has intrinsic value, and only what acts as a whole, that is, is a functional whole, is a real whole. But what indicates the bounds or limits of such a whole? A true whole, a genuine person is when there is not only activity starting from it, but when activity starting from it also in its aim in some manner comes back to itself. Such "self-determination" is the philosophical criterion of person and of intrinsic value. Stern holds that he here arrives at a union of the ideas of being and of value, and supplies a test for judging any claim to absolute value.

The act of valuation affirms the existence of values: the values of the persons valuing and the values of the objects valued. It is impossible to avoid recognition of a plurality of values, or, in other terms, of persons. It is, however, only by a metaphysical paradox that one may talk of a system of values. For the nature of a system is such that all its members are related: and the nature of intrinsic values is that they are dependent solely on themselves. Here, says Stern, we are at one of the crucial points of metaphysics, where the concepts of the intellect no longer suffice. What is not logically explicable must nevertheless be accepted as a fundamental fact of the world:—that some existents may at one and the same time be absolute and relative, have both intrinsic and instrumental value. The term "system" is not satisfactory for such an "overlogical" combination, for a system ought to be capable of reconstruction in thought, but this whole is open only to metaphysical conviction and intuition. The paradox is also apparent with reference to the problem of the gradation of intrinsic values. For gradation implies relativity to a standard, a sort of *absolutissimum*, to which values, themselves absolute, are related. Stern meets these difficulties in part by the conception of a hierarchy of intrinsic values, or more correctly, a hierarchy of persons having intrinsic values. The exact relation of persons in such a whole cannot be adequately comprehended intellectually, but Stern insists that a whole need not shut out other wholes from itself: it may include them and their intrinsic values. *A priori* one arrives at the conception of the highest stage of the hierarchy, the All-person. One implication of this hierarchical arrangement is that the distinction between person and thing has only relative existence in the realm of facts: it is a distinction of ways of looking at the same set of facts. For in the region of the finite, every whole is part of a higher whole, and to higher persons is

"thing". But the lower wholes do not lose their own intrinsic values by their inclusion in higher wholes. The concept of the All-person is reached *a priori*, but the intrinsic values or persons of the different stages of the hierarchy are to be known empirically. Except for the important consideration that Stern does not stress the psychical aspects his enumeration of the stages of this hierarchy is suggestive of Paulsen.

The gradation of intrinsic values, otherwise the positions of persons in the hierarchy, is said to depend upon the extent of the "fullness of intrinsic value" as compared with the All-person. From this notion of "fullness of intrinsic value," which Stern calls a category, one may pass to his distinction of "real" and "ideal" intrinsic values. As the person is a *functional* whole the nature of its intrinsic value can be clearly understood only through its openness to the world. But for the individual person the world has a double sense: as a system of forces in which it has its place, receiving and giving; and as a hierarchy of intrinsic values into which it must fit itself and towards which it must take up its position. It is to the latter that the conception of ideal intrinsic value has reference. Real intrinsic value is the person as actual, the being living with its inner striving and its external influences. Ideal intrinsic value is a whole conceived as complete or perfect—not now a reality but a tendency to realisation. Each person is a centre of the cosmos of intrinsic values, and each has a common relation to the value-universum, and this determines in some manner the ideal intrinsic value for each. Through its connexion with the value cosmos, the individual person has its religious rank and its ethical direction.

The intrinsic value of a person is a quality of the whole as such, yet all that belongs to the person has some sort of share in that value. Nevertheless the part is something other than the whole, for in the part only a side of the being comes to effect. There is a tendency for the value in the part to be looked upon as merely of the status of instrumental worth. From another point of view the whole is present in the part, and then there is a tendency to treat values therein expressed as intrinsic. Nevertheless such values are neither intrinsic nor instrumental: they have no independence of the person as a whole; and they are not simply instruments but constituents of the person. These are the intermediate values, the *Strahlwerte* in Stern's terminology; the Constituent Values as it has been proposed to render that term. They are constituents of persons though they have not the self-determination and being characteristic of persons. Intrinsic values are said to flow in and to express themselves in Constituent Values.

From the metaphysical standpoint of Critical Personalism, the distinction of physical and psychical is not fundamental: the person is both. At some length, but proportionately indefinite and vague, the author endeavours to distinguish his view from that of Spinoza and from modern forms of psycho-physical parallelism. The

psycho-physical distinction has a central significance in the realm of Constituent Values. Having considered the forms of modality of Constituent Values and the question of their gradation, Stern passes to a description of the main types of these values. Consciousness, only a state of a person and not independent being, is a Constituent Value. Its treatment as independent being has led to the error of regarding it as of intrinsic value. Stern believes that the discovery of the "Unconscious" has been fundamental for metaphysical theory and psychological knowledge, and is perhaps still more significant for the valuation of the world. But the Unconscious includes both that which is "over" and that which is "under" the conscious. One may speak of the unity of the person, but not strictly of the unity of consciousness, for this in order to attain its significance has continually to draw upon the "over" conscious. In his treatment of the various Constituent Values associated with self-consciousness, the person's experience of itself as "I," Stern discusses the status of feelings of pleasure and pain. He seems to hold that both are inseparable from individuation. They betoken the conflict through which the person comes to self-consciousness. He makes the very debatable assertion that pleasure requires for its existence both the existence of the capacity to suffer and the existence of actual suffering. Nevertheless, pleasure and pain are qualitatively different and cannot be quantitatively measured one against the other. And yet, in spite of the apparent correctness of this, may it not be asked whether there is not, in some theoretically inexplicable manner, in practice a sort of commensurability of all values, some sort of summation and comparison of totals?

Truth, regarded by Abstract Idealism as of intrinsic value, and by Pragmatism as instrumental, is a typical example of what Stern classes as Constituent Values. It involves as presupposition the intrinsic values of the knowing persons and of the objects known. Nevertheless, it is not a constituent of either alone, but of a unity in which both are present. There is a multiplicity of truths in the different stages of the hierarchy of persons. As expressed through ideas representing constituents of higher unities, they have an objectivity and something of the normative for persons lower in the hierarchy. It is difficult to surmise what might be Stern's theory of error, which he entirely fails to discuss. There is an important chapter on the historical. Persons, particular wholes of intrinsic value, may be finite in time, have a beginning and an end. That appears to be his view as to particular human persons, and even of humanity, but there is much which is barely consistent with this, and he points later to a conception of God in which all persons, all intrinsic values, find their completion, perfection, and persistence. There is astonishingly little consideration of the æsthetic. It is interesting to observe that even in the brief account of Instrumental Values, as throughout the book, the concept of "thing" plays practically no part.

In a third part of the volume Stern elaborates his views on "Introception," by which he wishes to express the active relation of the individual person as real intrinsic worth to the Value-cosmos. Introception is contrasted on the one hand with theoretical subjectivism and practical egoism, and on the other with theoretical objectivism and altruism. In Introception the I makes the affirmation of the values of the not-I a constituent part of its own intrinsic value. The fundamental forms of Introception are love, in its types as erotic; as in the family and in wider social wholes; "understanding knowledge," an intellectual comprehension which is something more than theoretical science; æsthetic appreciation and expression; and religion. The final chapter treats of morality from the point of view of Critical Personalism, showing the meaning of Introception as the fundamental ethical ought. As contrasted with Kant's "I ought, therefore I can," Stern stresses the importance of the implication of "I can, therefore I ought," but he appears somewhat inappreciative of the confusion which that as a maxim might bring into human conduct.

Enough has been said to indicate Stern's general position. He makes a valiant attempt to elaborate a metaphysic which shall represent a unity of a theory of being and a theory of value. But it must be said that, apart from intimating their common relation to the person, he is no more successful in showing the nature of the unity of values than the philosophies of culture which he condemns. His neglect of the side of the value-problem which may be called the evil, especially error, ugliness, and immorality, is a serious defect.

ALBAN G. WIDGERY.

The Sensory Basis and Structure of Knowledge. BY HENRY J. WATT, D. PHIL. Methuen & Co. Ltd., 1925. Pp. x + 240. 8s. 6d.

THE death of Dr. Henry J. Watt since the publication of this book deprives Psychology in this country of one of its most distinguished experimentalists. Whatever may be the fate of the system he was endeavouring to construct his writings deserve close attention on many grounds, particularly, perhaps, for the sake of the care and accuracy devoted to the discussion of empirical data. Watt was an unusually accomplished introspectionist and possessed an amazing fund of knowledge concerning the facts of sense experience.

His writings, from the earliest papers to the present work, reveal a remarkable singleness of purpose. Apparently quite early in life he formed a definite conception of the ideal towards which he thought a scientific Psychology should aim, an ideal frankly modelled upon other empirical sciences, particularly that of Chemistry. In all his writings he is attempting to render the foundations of such a Psychology more secure.

His point of departure was amidst the ruins of the old Associationist theory where, as he explains in an early article ("The Elements of Experience," *Brit. Journ. Psych.*, Vol. IV.), he found two parts intact and firm; the elements of sensation and the bond of association. Whilst others attempted reconstruction by recognising new elements Watt, accepting sensations as sufficient, attempted to renovate the binding material.

The bonds of association and of mere aggregation are supplemented on Watt's theory by that of "integration," a mode of synthesis in which in addition to the participating elements there emerges a new factor dependent upon the integrants but which we could not by any logic deduce from their attributes.

By integration we pass by a succession of stages from the elementary sensations up to all the "higher" functions of mind. The basic elements are not the crude sensations of casual introspection but determinate units, "particles" to which ordinary sensation can be reduced. These sensory particles correspond to the unit neural process arising from stimulation of isolated "spots," the spot system being the uniform physiological basis throughout the whole range of the senses. Moreover these particles in every case approximate to a type completely definable in terms of six fundamental attributes: quality, intensity, extent, systemic position, duration and temporal position. Such is the admittedly meagre basis on which the whole structure of mind is reared.

From the isolated sensory particles we proceed to the first level of integration (or single sense integration). Lines, masses, distances, etc., issue from the integration of the systemic attributes; intervals, rhythms, etc., from the temporal; and motions, melodies, etc., from the integration of systemic and temporal together. The dissystemic integrations (those of two systems belonging to the same sense) constitute an intermediate link with the higher levels of complication in which one obtains integrations of integrates, such as fully developed spatial perception; and so on until sense "runs smoothly and continuously into intellect".

Feeling is not discussed in this volume, but in the article previously referred to an attempt had been made to bring this mode of consciousness into line with the general thesis.

An ingenious argument was put forward to show that feeling has all the marks of an integrate, and is in certain precise respects analogous to the experience of motion, differing, however, in having as basis quite other attributes from those upon which the experience of motion depends. It was the difficulty in determining of which attributes feeling could be an integrate which seemed to make the author sceptical of this earlier theory, for it is essential to an integrate that it should be founded directly or indirectly on these primary attributes. Quality was not considered a likely foundation. In fact, apart from such doubtful cases as that of "lustre," quality and intensity are believed to show no signs of integration.

There are many grounds on which the author's system will be

attacked, but some of the criticisms coming first to mind are the most difficult to sustain. Thus the assumption that the sensory particles are mental entities, though explicitly adopted, has no relevance to his major thesis. The whole theory of progressive integration would be equally consistent with a form of Neutral Monism. Moreover, Dr. Watt has carefully guarded himself against being confused with the older sensationists. Even the 'mental act' which is absent in the beginning seems to come in, with a different analysis, as a product of integration.

The vital point of the theory, however, is of course this notion of integration itself. It is difficult to see what, after all, an integration can be but the fact that two terms stand in a certain relation; for it would fairly generally be agreed that the relation is something 'new,' is indefinable in terms of the intrinsic properties of the relata, and that it binds these relata into a unity. It is also true of any relation as of an integrate that it is founded in its terms.

But on this interpretation difficulties arise. To speak of position as a primary attribute and of distance as something in some sense derivative from differences of position inverts the order of logical priority, and of psychological priority as well. If, as it is supposed to, the theory is to provide a foundation for a *genetic* psychology this priority of primary attributes over the integration is required; and position and distance do not appear to be so related.

Again, on this interpretation, why are the systemic and temporal attributes credited with the peculiar power of generating integration? According to the author simply because empirical evidence of integration based on qualities is lacking. Surely, however, when red and green patches are presented in different positions not only is the distance given but also another new datum—their 'contrast,' as also is their "distance" of quality difference, as compared with another distance between red and pink. Why are not these also called integration? The fact of the matter seems to be that not any relation is sufficient to constitute an integrate but only those which 'bind the integrants' into a whole, or which are members of that class of relations which together constitute the "unity of the mind". But nowhere do we find a quite clear definition of integration which does not raise difficulties with regard to some or other of the special modes of synthesis to which the term is applied.

In spite of the difficulties in following the author in his general thesis interest is found continually in his detailed discussions on the way. There is for instance an extremely conscientious attempt to formulate a precise and consistent doctrine of psycho-physical parallelism, which is relevant on any theory to the question of the relation of sensory appearances to cerebral concomitants. The author is of course freed from the embarrassment of a multiplicity of ultimate modes of consciousness, and from the distinction of act and content, and has therefore only to correlate his particles, their six attributes and later their integrations with some possible neural processes.

To the sensory particle there is postulated a corresponding unitary neural process. A change in systemic position might correspond to a change of locus in the process in the brain. A change in extent (being a change in the number of neighbouring particles) would correspond to the number of neighbouring unit processes. Sensory intensity corresponds similarly to a change in physical intensity in the neural process, definable ultimately in terms of density or speed of process and the summation of stimuli.

For so much hypotheses are readily at hand. In finding correlates to changes in quality and the temporal attributes the author admits difficulty, though in respect of the latter he seems inclined to adopt a simple parallelism between the temporal attributes of sensation and the characters of real physical time in their application to physiological processes.

"Quality," however, "is the attribute which gives us least hope of insight into itself." . . . "It looks like something quite new, something that matter could hardly produce by itself; for as far as we know all differences of matter are reducible to differences of spatial arrangement and movements of particles of one or more unknown kinds."

The point on which the parallelist has at the outset to make up his mind is whether he is going to insist on differences in kind in the physical world to correspond with differences in kind in sense experience, or whether he is prepared to admit that the latter may quite ultimately and irreducibly be paralleled by differences of degree or of spatio-temporal arrangement only—as in the purely psycho-physical parallel between external stimuli and sensations. If the former alternative is adopted there is nothing for it but the desperate expedient of postulating differences in kind in matter of a type unknown to natural science, these differences manifesting themselves only in effects upon sensation.

The second alternative seems simpler—in which case it is easy to conceive some differences of quantity or arrangement to parallel differences of quality, though the parallelism will remain to the end of what Prof. Stout describes as a "brutally empirical nature".

At the brink of his solution, however, Dr. Watt becomes paralysed by philosophic doubt. He writes (p. 71): "Strictly speaking (or philosophically), we must recognise that none of the attributes is any worse or better than any other. Time only exists mentally, as an individual 'thing': physically it is an unknown quantity that separates, or rather, is implicit in events (as they are reconstructed in independence of any particular point of observation, by the theory of relativity). The same is true of space. So the mystery of quality seems less oppressive." . . . The difficulty seems that the author has not quite made up his mind whether to approach the problem at the level of "enlightened common sense" or at that of a more radical philosophy. In a final reference to the question in the concluding chapter the author hints at a kind of philosophical agnosticism. In virtue of parallelism (which is here

causally interpreted) the pattern of processes in the external world "pass into the mind". We "know the external world only as a set of unknowns in certain relations, these relations being embodied in those systems of mind that are best capable of taking them over".

Dr. Watt had continued to the end ploughing a lonely furrow, which the main body of psychological workers had forsaken. But his fidelity was not in any way due to any blindness to the difficulties of sensationism. It was, rather, due to a stubborn determination to resolve them. Though he studiously avoids polemical discussion he appears to have been fully aware of the work of others approaching the problems from entirely different assumptions, with the consequence that he has much to say of general interest. His style is often obscure but this book abounds with valuable contributions on empirical points of detail. It is one of the few of which it can be said that it would have been proportionately better had it been twice as long.

C. A. MACE.

VI.—NEW BOOKS.

Miracle and its Philosophical Presuppositions, three lectures delivered in the University of London, 1924. By F. R. TENNANT, D.D., B.Sc. Cambridge University Press, 1925. Pp. 103. 4s. 6d. net.

The belief in miracles presents problems of the greatest interest, not only to theologians, but also to scientists, logicians, and psychologists. Theologically, miracles raise the question whether and how they are a support of a particular theology, and just now a growing number of Christian theologians are inclining to answer that they are a superfluity, an embarrassment, and even a hindrance. Scientifically, they raise the questions whether the belief in the 'uniformity of nature,' the absolute reign of 'law,' is well founded and how any cosmic order can survive their occurrence, questions which cut very deep into the foundations of science. Moreover the logician is entitled to observe that all these questions have a logical side, and generate very pretty problems as to the logical interrelations of their various answers. He must therefore endeavour to estimate the logical import of the belief in miracles. While finally, to the psychologist, this belief is a clue to a characteristic region of human mentality, and to much of human history.

We have therefore every reason to welcome a treatment of miracles by a scientifically trained theologian, especially by one who thinks so clearly and tersely as Dr. Tennant. He begins by disposing of the old idea that we can get to know 'laws of nature' so absolute that they render 'miracles' impossible. The pragmatic theory of science easily shows that a 'law of nature' is only a human formula for describing the habits of nature observed up to date, and a postulate so suggested, but incapable of yielding any absolute guarantee of the future, and liable to be superseded at any moment by a better formula which will embrace and cover the events previously rejected as miraculous. *A priori* rejection of testimony to a rare and abnormal event which is interpreted as a miracle thereby becomes indefensible.

But the new theory of knowledge not only knocks out as 'pseudo-scientific' what had ranked as the most potent disproof of miracle; it also affects the proof of its occurrence, and brings out a latent ambiguity in the conception. Is 'miracle' to mean an event referable to supernatural agency, or merely a 'sign' or 'wonder'? In the latter case it may well owe its impressiveness to a 'law' which was at the time unknown, and may be really ascribable to 'natural' causes; it becomes therefore impossible to prove that the agency concerned was really supernatural. And then what becomes of its evidential value for theology? "Science thus leaves theology free to assert the possibility of miracle; but she seems to preclude the possibility of our being able to pronounce a marvel to be a miracle in the objective or absolute sense of the word" (p. 32). Again if "supernatural causation means fresh, direct, unmediated or non-devised, intrusive or interpolated, activity on the part of the Creator" (p. 48), how can we ever prove that we are *right* in interpreting an event

as due to this? "We seem to be as far as ever from being able to know that any given event, however marvellous, is actually a *miraculum*" (p. 61). "Miracle, while possible, becomes unknowable or unrecognisable if it actually occurs. The evidential value once ascribed to miracle, and which gave the miracle its theological significance, has disappeared. In so far as establishing theism is concerned, it has become evident that belief in miracle presupposes belief in God" (p. 62). Thus miracles do not prove revealed religion which "presupposes rational theology" (p. 63), and miracle at most "suggests divine activity, just as the success of science suggests that the postulates underlying induction are true" (p. 66). Faith-producing is the sole function left to miracle, and instead of logical certainty we get psychological (p. 74), or rather "religious impressiveness" (p. 76). From this situation Dr. Tennant deduces that "though miracles may occur we have no right to believe in their occurrence" (pp. 82-83) in a strictly objective sense, and "all discussion of the antecedent probability of miracle is futile" (p. 89), because "the whole matter is thus psychological" (p. 87). So Dr. Tennant arrives at the conclusion that "alleged miracle is devoid of all evidential value. Christianity does not presuppose Christian miracles; they presuppose Christianity, though they are by no means bound up with Christianity" (p. 94). The whole controversy "has become of but historical interest" (p. 95).

This conclusion appears to me something of a *non sequitur*. It stultifies much of Dr. Tennant's argument, and I would humbly suggest to him that he might have done better to infer from his analysis of the situation that purely objective and logically coercive proofs of miracles are as unnecessary as they are impossible and that 'religious impressiveness' is all that is wanted. For after all though not forced to believe we retain the right to believe and the wish to believe, and nothing more is psychologically necessary. It is precisely their impressiveness which sustains the popular belief in miracles, and it is the business of theology to understand it before it abandons it. The apparent paradox to be explained is that the human mind can simultaneously hold both that the order of nature is good and divinely instituted, and also that it may be all the better for divine interferences which upset its routine. When a psychological explanation of this attitude can be given, it will probably be found that its logic also is not utterly contemptible, though it may jar upon certain theological prepossessions. But for this theology may be to blame, which has not, certainly, been very successful in showing why an omnipotent deity should both institute an order of nature and also find it expedient or necessary to upset it by miraculous interventions. *Prima facie* it seems more plausible to argue that 'law' and 'miracle' cannot both be derived from the same source: either the order is divine and the miracle diabolical, or the miracle is divine and the order diabolical. Dr. Tennant just mentions this difficulty (p. 90), but dismisses it as mere 'rationalistic prejudice,' whereas it is really the dilemma which reveals the incoherence of rationalist theology. Nor is it a solution to plead that evolution is 'epigenetic' or 'emergent'; for these are merely phrases for the generation of *novelties*, and novelty as such cannot be scientifically explained, and means the same as miracle.

Dr. Tennant's somewhat lame conclusion thus seems to be due to his undervaluing the psychology of religion. But he may be forgiven much in view of his sturdy theism and unequivocal rejection of the Absolute (p. 39), while the Law of Excluded Middle, deliciously suggested to him by a printer's devil (p. 70), is certainly an addition to logic which deserves to meet with universal acceptance as one of the prime necessities of thought.

F. C. S. SCHILLER.

Spinoza's Ethica, Analyse und Charakteristik. Von HARALD HÖFFDING. (Bibliotheca Spinozana, Tomus IV). Curis Societatis Spinozanae, MCMXXIV. Oxford University Press. Pp. 146.

The first two sections of this commentary to the *Ethica* of Spinoza have already been published in the yearly volumes of the *Chronicon Spinozanum* for 1921 and 1922 respectively, and the work as a whole now appears as noteworthy, less as an advance in Spinoza scholarship than as embodying in brief and useful form the balanced interpretation of Spinozism which is now widely accepted among students.

In his opening chapter on 'Die drei Gedankenmotive Spinozas' Höffding traces in masterly fashion the main sources from which the finished product of the *Ethica* was derived: the training in Jewish theology leaving its indelible mark upon the keen young mind of the budding Rabbi; the new science appealing with irresistible force to his developed intellect; and, with these, something innate, the intellectual and religious need so vigorously described in the opening pages of the *Tractatus de Intellectus Emendatione*. In the *Short Treatise on God, Man, and Human Well-being*, to which Höffding makes frequent and fruitful reference, we see that spiritual need co-operating with theological conceptions to produce an immature foreshadowing of the *Ethica*, a "peculiar mixture of mysticism and theory of knowledge," in which God is the ultimate principle of explanation, and the bias towards intellectualism is already fixed. The ultimate reality must be something indubitable, that is to say it must be related to the world as proof is related to reasoning; not something extraneous to it but the condition of its validity and intelligibility. God is one with the lawabidingness of nature. And this early theological cast of thought conditions the appeal of the new science to Spinoza. Like Newton he regards investigation as divine service; for when we understand phenomena what we grasp is not the phenomena themselves but their lawabidingness, their 'divinity'. Such an attitude of mind cannot but imply a peculiar personal mood or need: a reflective mysticism it might be called, but it is remote both from the rationalism of the Enlightenment and from ordinary mysticism; it is a mystical view of the world transformed into rational form. No less than this underlies the wellworn 'Deus sive Natura'.

It is neither possible nor desirable in this slight notice to do more than emphasise one or two outstanding features of Höffding's interpretation of the *Ethica*. It is refreshing to find a commentator at length applying to Spinoza's own treatise the familiar epistemological distinctions of the *Tractatus de Intellectus Emendatione* and the *Ethica*, and recognising that the *Ethica* is itself only a product of the second kind of knowledge, Ratio, and is therefore necessarily abstract and so far unsatisfactory. It can neither descend to the particular nor ascend to a full apprehension of the concrete nature of Substance. Hence its emphasis upon the lawabidingness of nature and the tendency to give an abstract account of Reality. I cannot but regard it as surprising that in view of his own statement Höffding does not appear to recognise that the third kind of knowledge must reveal an Ultimate Reality which is very much more than "the lawabidingness of nature". I suggest that a firm grasp of this distinction will do much to account for Spinoza's repeated efforts after the concrete universal and as frequent lapses into the abstract.

No student of Spinoza for very long escapes the puzzles that have centred about the doctrine of the infinite attributes, and Höffding makes repeated references to that doctrine, and wrestles with some of the problems. In particular he objects to the "nihil aliud" of *Ethica II., xiii*, on the ground that all the infinite attributes correspond, and the idea of the

human body is therefore necessarily also the idea of (say) x , a mode of some attribute other than *Extensio* and *Cogitatio*; it is, in fact, the idea of infinite modes, one in each infinite attribute. I suggest, however, that this by no means follows since it is the essence of *Cogitatio* to be the reflection of *Extensio*, and *nothing else*; otherwise thought is coextensive not with *each* other attribute but with *all* other attributes. That has, I am well aware, often been asserted by commentators, but it can hardly have been Spinoza's own view. The correspondence of the infinite attributes need not be, indeed cannot be, that of idea and ideatum. If it is asked: are the other attributes, then, unknown even to God? the reply must be threefold: firstly, that we know nothing about God's capacity to know over and above the attribute of thought; secondly, that God's apprehension of the other attributes is not through *Cogitatio* though it may be through another attribute corresponding with but not identical with this (for example through *Enjoyment* or *Power*—though this is to go beyond our book); and lastly, that the Divine Intellect may comprehend infinite types of apprehension other than *Cogitatio* and so may differ from our minds as "the Dog, the heavenly constellation," differs from "a dog, the animal that barks" (*Ethica* I., *xvii*, *Sch.*).

The third and last feature of Höfding's treatment of Spinoza to which I desire to refer is his sympathetic appreciation of the philosopher's treatment of Value. I cannot but regard this matter as vital for Spinoza's position as a philosopher, though it does not touch his credit as a psychologist or as a natural philosopher. No part of Spinozism has received more destructive attention than his treatment of worth and purpose, of good and bad, beauty and order. The destructive criticism of final causes in the Appendix to Part I. of the *Ethica*, has led too many students to believe that Spinoza wholly rejects all that we include under the term Value, and that he is, in effect, therefore, a philosopher without a philosophy. Nothing could be further from the truth. Undoubtedly he rejects all measurements of worth which are based upon the relationship of things to human needs and desires; he refuses to accept explanations of single special phenomena in nature by reference to the idea of value; in these rejections he may or may not be justified, but they did not for him imply the rejection of the ideas of Value and Perfection. He rejects a "peddling Providence" because he believes so profoundly in the activity of God. Höfding sees this very clearly, and states it with enthusiasm. Spinoza, with Plato, identifies perfection with reality and existence, "*Perfectio rei existentiam non tollit, sed contra ponit*" (*Ethica* I., *xi*, *Sch.*), "*Per realitatem et perfectionem idem intelligo*" (*II*, *Def. vi*). Nor is the æsthetic element lacking: it takes the form of deep wonder at the perfection of a world which can produce the multiplicity of particular things, great and small, good and bad, useful and harmful, at the fullness of the Deity for whom "matter was not lacking . . . for the creation of every degree of perfection from highest to lowest" (in the phrase of the offending Appendix). Höfding also refers to the autobiographical hints of the *Tractatus de Intellectus Emendatione* and points out what is the real peculiarity of Spinoza's conception of value, viz., that all worth is for man summed up in truth, and man's highest good, the intellectual love of God, is based on the third and highest kind of knowledge. This exclusive intellectualism is, of course, widely condemned at the present time, but if the aim of philosophy as a form of knowledge is the intellectual apprehension of reality, it is perhaps hardly a fault in it that it is intellectualistic. We may or may not apprehend value by means other than the intellect, but what is not intellectually apprehended cannot be subject matter for philosophy as a species of knowledge.

The *Societas Spinozana* has been very fortunate in securing this

volume for its *Bibliotheca Spinozana*. Dr. Höffding has long been recognised as no mean Spinozist, and he has brought all the wide resources of his knowledge of both the early and the mature work of the philosopher, as well as his own great philosophical acuity, to bear in this the latest product of his fertile genius.

H. F. HALLETT.

L'expérience physique chez Roger Bacon : Contribution à l'étude de la méthode et de la science expérimentales au xiii^e Siècle. Par ROGER CARTON. Paris, Vrin, 1924. Pp. 189. 12 fr.

L'expérience mystique de l'illumination intérieure chez Roger Bacon. (Same author.) Pp. 376. 20 fr.

La synthèse doctrinale de Roger Bacon. (Same author.) Pp. 376. 20 fr.

These three works are an attempt to do for our English doctor what has been done for the other great teachers of the thirteenth century, to systematise his teaching and to bring out its characteristic points. The first of them, which is issued with the imprimatur of the University of Paris, attempts to define what Bacon meant by experiment and certitude; the second is a study of the mysticism in his writings, and his intuitionist theory of knowledge; the third is devoted to the aim he set before him. I am not sure that the time has yet come for such a study. We have had the complete works of St. Thomas and St. Bonaventure and of many lesser thirteenth century teachers available for centuries; in the case of Roger Bacon the larger part of his work is still unpublished, and that part of it available to students has not been subjected to critical study. Most of the *Opus Minus*, the more important part for Dr. Carton's purpose, still awaits publication by Mgr. Pelzer, as does the remainder of the *Metaphysica* also found in the Vatican eleven years ago. The Amiens manuscript, of which a first instalment is now in type, is absolutely necessary for the discussion of Bacon's theory of intuition, and even much of the *Communia Naturalia* exists in two other unprinted versions showing substantial differences between each of the three forms. Even when the whole of the manuscripts are in type, a critical study of dates and the history of the texts will remain to be made before the task of the commentator can be safely begun; a fact which Dr. Carton himself seems to recognise.

However, a large body of comment on Bacon's philosophy is already in existence, and our author has considerable justification for adding to its bulk. He has made a conscientious study of the printed works of Bacon and of the literature on the subject, he is well-versed in the philosophy of St. Thomas and of St. Bonaventure, and he has enriched his text with copious references. Unfortunately what he has to say is by no means clearly said—sentences of four hundred words or so (e.g., pp. 8, 9, 10) are hardly in the tradition of the French language, at any rate till the coming of Proust—and his method of sometimes stating a position in the person of Bacon and then reverting to his own comment does not make his meaning any clearer. Without the summary of the arguments at the end of each volume we should often be lost in the mass of verbiage.

The parts of this trilogy which suffer least from the paucity of material are the first and the third—the examination of the experimental method of Bacon, and of his conception of the object of science. The nineteenth century was attracted by his statement of the importance of experimental science, and his insistence on verification: twentieth century scholars seem to be approaching the conclusion that his science was not experimental, and his verifications intuitionist. Now it is no doubt true that

experimental science, in our sense of the word, was unknown in the thirteenth century, and we may admit that even one familiar with the whole body of Bacon's writings would find it difficult to produce many examples of actual experiments, described in so many words, but it must be remembered that the works on which modern criticism of him are based are not primarily treatises on science at all. The *Opus Majus*, *Minus*, and *Tertium* are persuasions, addressed to the Head of a Christendom menaced from the East by the terror of a new invasion and oppressed by fears of the coming of Antichrist. They were intended to put before him the resources of science and art, to urge him to take them into his own hands and to gather round him a band of scientific men. It is this purpose which explains Bacon's insistence on utility to which Dr. Carton so often reverts: it was the means of combating these perils that Bacon was ordered by the Pope to indicate. It would be well, too, to remember that at that time the accepted test of a science, old or new, was not its truth, but its conformity with the rules of the science of syllogisms, a doctrine emphasised by the greatest teacher of Christendom, Albert, in his comment on the *Posterior Analytics*. As to Bacon's position as an experimenter, it is true that a man may spend £2000 on books, experiments, instruments and tables without getting much result, but no one can read his work on optics without feeling the experimental work behind it, his astronomical work on the verification of the tables involves skilled and constant observation, while his genuine alchemical works, for which we have a criterion in the still unpublished part of the *Opus Minus*, could hardly have been written as mere copies of previous works come into his hand without verification, nor would he have dared to press that science on the Pope with such urgency. The hermetic side of Bacon's science is not a corpus of scientific knowledge set out for universal use, it is an indication of the power that science can put into the hands of the master of the world. Dr. Carton's insistence on illumination as the source of knowledge and the true equivalent of experience in Bacon's teaching is based on insufficient grounds. Let us use these letters to the Pope, compiled in a hurry from materials new and old, for what they are worth, and not extract a system of philosophy from a persuasion in which it is taken for granted. When in the course of a year or two his questions on *Metaphysics* and *Physics* are in the hands of scholars, we may expect from Dr. Carton a fuller and more complete study, not so much of Bacon's experimental method, which should be written in a language which has different words in common use for 'experiment' and 'experience,' as of his psychological theories: at present the basis for such a study seems to us insufficient. The second book, re-written and enlarged, would be an admirable foundation for one.

R. S.

Les Valeurs Affectives et L'exercice Discursif de la Pensée: Essai d'Empirisme Psychologique Radical. PAR DANIEL BERTRAND-BARRAUD. Paris: Librairie Philosophique J. Vrin, 1924. Pp. 306. 22 fr.

Monsieur Bertrand-Barraud propounds an original thesis in this elaborate work. He seeks to found a consistent account of reality as well as a thorough psychology upon affective experience. In his view rational Idealism on the one side, and scientific Realism on the other, have failed to give due place to the feelings in experience. A detailed examination of consciousness reveals an immanent psychological movement, a qualitative deployment of satisfactions or intrinsic values, discoverable in their essential features in the primitive and immediate

consciousness as in the most developed mind. The theory of knowledge which such a psychology unfolds is distinctly Kantian; the forms of representation are necessary and critically immune, though operating only in experience. But a pragmatic direction is given to the orthodox tradition by recognising that the logical constitution of the subject is a tool and one that adapts its shape to its effective use. This view, however, does not lead the author to a voluntarist creed, a line of thought which in more than one place he uncompromisingly rejects.

The main portion of the book is divided into two sections. The first gives a sketch of a concrete psychology; the second discusses the dynamism of the spirit. An analysis of the immediate field of consciousness is first presented. Two classes of affective experience or values are recognised. The first includes a number of purely affective states—sensations, feelings of pleasure and pain—and, at a more complicated level, emotions and sentiments. Attention is drawn, moreover, to the affective value of ideas and concepts: "Pourtant notions, concepts, mots, sont, en fait, appréhendés d'un coup et incorporés dans les phrases tout comme les sons, les couleurs, les saveurs ou les odeurs se fondent avec leur contexte sensoriel, car, en ce moment même, ceux dont j'ai besoin s'agglomèrent et acquièrent un sens pour moi, de même que si je me promenais, des groupes d'impressions visuelles prendraient la signification perceptive d'une maison, d'un chapeau, d'une dame." (p. 62).

The second group is termed intellectual values, and is closely woven, wherever consciousness is present, with the pure affective values. The intellectual values comprise the operations of reference, attention, representation. From bare awareness to discriminative attention the development is one of degree, not of kind. But what is more important, attention is itself a kind of specific feeling; for every state of consciousness is in itself a real psychological value (p. 67). Intellectual value is the instrument by which the given is appropriated. Knowing is a kind of double feeling. At a more complex stage the fusion of intellectual value and feelings appears in the formation of notional sentiments, ready to intervene in the process of thought. And this intervention is not regulative only, it is creative (p. 100). Perception, judgment, conceptual reflection, display original amalgamations which are spiritual creations. More strictly, the act of judgment permeates all levels: "In speaking of the act of judgment, what is expressed is simply the fact that a new affective-intellectual complex has been formed, that a new manner of being, having intellectual rank, has been constituted" (p. 277). An especially interesting section is devoted to an examination of the relativity of sensation; the errors associated with which are traced to the dogma of psychological passivity. For such a view permits of the application of transitive causality to mental processes. It is to substitute life conceived as a series of discrete states for life actually experienced (p. 94).

M. Bertrand-Barraud's vigorous criticisms of psychological abstractions should meet with sympathy. The close touch he preserves with unprejudiced introspection results in a psychology which rejects from a new angle the divisions of tradition. But while many perhaps will agree with him in his insistence on psychological activity, even in sensations and in pleasure and pain (p. 78), the nature of this activity by reason of the author's dismissal of a conative point of view becomes somewhat obscure. His doctrine that the consciousness of the rational subject results from the constant fusion of affective values with intellectual sensibility (p. 135), in spite of the care with which he develops it, seems to demand some further explanation. Probably the author's meaning has not been fully grasped; but the nature of intellectual value which gives unity,

uniformity and permanence (135) to the purely affective values, seems to point to a more metaphysical belief than the author is inclined to adopt. It is hardly sufficient to describe spatial reference as a sort of style conferred by the representative process on psychological activity (p. 65).

The second part applies the doctrine of affective values to various sides of experience. "The metaphysical constraint of the causal relation" (p. 138) is removed after a discussion of Hume and his critics.

A rejection of activity in itself follows, and the way is now clear for an examination of the feelings and sentiments which ordinarily move men. The outcome is to justify the faith of the Savoyard Vicar—"Notre sensibilité est incontestablement antérieure à notre intelligence." The teleological character which is given to these more or less amorphous states of sensibility ready to become articulate in appropriate situations, emotional, intellectual, æsthetic, is illusory. But what leads thought into new paths is not a cognitive but an affective element. This idea is excellently worked out in the section on the artistic sentiments and the creative imagination, and the sentiments which rule practical skill are clearly contrasted with automatic action by means of striking examples.

The principal feature of the author's theory of the discursive exercise of thought is that verbal forms not only express thought, but bring it into being. "... nous soutenons que la pensée discursive, mode du dynamisme affectif, a pour origine l'intervention des mots. Ceux-ci n'expriment pas une pensée intellectuelle préexistante, mais ils la créent en collaboration avec les affections. . . ." (p. 217). The scarcely apprehended complex of feelings which flash into articulate expression by synthesis with a verbal image, are not formed of associations between psychical groups recognised as distinct. Feeling the experiences as a unity precedes the abstract reference. The mind is an intricate organisation not of empty concepts, still less of physiological groups, but of notional sentiments which conserve the past and which by their intrinsic value produce the stream of ideas. And trains of thought are sustained and promoted by the living conjunction of a verbal sign and massive impulses which sub-sist in the mind. What matters is the concrete context and in developing this the mind is guided by no abstract rules; whether the principle of identity or any other (p. 238).

M. Bertrand-Barraud's grand attack on "intellectualism" cannot be justified further here; and there are large divisions of his work to which no reference has been made. That he places more weight upon the affective side of experience than it can bear seems very probable; there is no doubt that his analysis of the mental processes of everyday life is wonderfully fresh, and inciting to closer study of his whole position.

M. H. CARRÉ.

L'Année Psychologique. Vingt-Quatrième Année. Paris, Librairie Félix Alcan. Pp. 690.

The present number contains eight original papers with the usual invaluable survey of current psychological literature, a detailed description of a new apparatus for the measurement of tactual sensibility, and an account of the Seventh International Congress of Psychology at Oxford.

The first paper, on the "Psychophysiological Problems of Time Perception," is by the editor himself, Professor H. Piéron. It might be regarded as a continuation of recent papers on the time relations of sensation from the same pen. The author discusses the elementary perceptions, upon which the concept of time is founded, from the scientific or objective point of view, that is, from the point of view of the "specific reactions of man

to modalities of physical phenomena coming under the rubric 'time' (réactions spécifiques de l'homme aux modalités des phénomènes physiques appartenant à la rubrique 'temps'). His aim is to suggest the problems of time perception which may be experimentally attacked. That is to say, he sets out to sketch a programme of experimental work in this field, rather than to attempt a solution of the problems of time perception on a more or less speculative basis. The whole memoir is of considerable interest and importance. Starting out from the results of experiments performed by himself with the object of discovering the laws of the 'time of sensation'—results published in *L'Année* in recent years—he passes in review the main problems of simultaneity, succession, and duration of movement, and of change, from the psychological point of view, indicating the problems that have been studied, and those which still remain to be studied.

The second paper by Dr. Forster of Prague on "The Dynamic Theory of Colour Vision" is also important. Arguing from the fact of the identity of colour sensations obtained from different points of the retina, he comes to the conclusion that each fundamental colour has its 'seat' in a single centre of the nervous system. Central colour theories, as opposed to peripheral, have been formulated before, at least as a supplement to peripheral theories. Dr. Forster's theory is somewhat more elaborate, and more detailed, than most of these, but not different in principle. He proceeds to account for various phenomena of colour vision on its basis, notably the phenomena of simultaneous and successive contrast—in which case the account is very similar to that given by McDougall in terms of drainage of nervous energy—the Fechner-Benham phenomena, contrast phenomena in a smoothly graded disc, etc.

Two short papers on mental testing are contributed by Decroly. The first of these is on "Les Méthodes non-verbales d'examen mental". The author describes a non-linguistic test consisting of pictures in the relation of cause and effect, in two series, one (series A) representing effect, and the other (series B) representing cause. A picture of series A is presented to the subject and he must find the corresponding picture of series B. The pictures are capable of arrangement in an order approximately of increasing difficulty. Tables showing the results of applying the test to 289 children, ranging from age 8 to age 15, are appended. A series of group tests of a similar type for children at the period of passing from the infant to the primary school is also given. Some of the individual tests are American, and others are modified Binet tests. No numerical data are given. The second paper is devoted to a comparison of individual and group tests. Decroly begins by considering the disadvantages and advantages respectively of group tests. He then goes on to discuss the advantages of individual tests. The final conclusions regarding group tests are that their employment is justified when it is necessary to test a large number of subjects rapidly, or to compare classes or schools with respect to mental level, but for any fundamental intelligence testing individual tests must be employed. There is nothing very new in this.

In a paper entitled "Le Déterminisme de l'Emplacement des nids chez des Vespides" Prof. Étienne Rabaud continues his attack on the view that there is any purposive or prophetic factor in instinct. His contention is that, apart from general features determined by general conditions, specific features of the situation or form of wasps' nests are determined by local and immediate conditions, and wholly so.

Two of the other papers are of considerable general interest. Paul Kucharski in a paper on sensations of tone (*La Sensation tonale exige-t-elle plusieurs vibrations?*) describes experiments carried out at the Sorbonne with a view to answering the question whether several periodic

vibrations are necessary to give a tonal sensation, or only a single vibration, or a fraction of a vibration. He found that the height of a tone could be recognised from a single vibration, and contends that it is "the duration of each vibration that seems to be the cause of the specific sensation of tonal height," and that the repetition of the vibrations "may be considered as an accessory factor". Claude Bussard contributes a paper on automatic learning (*Recherches sur le dressage automatique*), describing experiments on dogs, rats, and guinea-pigs, with the object of studying automatic learning by a somewhat novel method. The animals were enclosed in a cage, of which the two halves of the floor were differently coloured. A coloured disc was exhibited, and at the same time one half of the floor was electrified, the animal being thus impelled to move to the other half. The method is interesting, but the paper can only be regarded as preliminary, and the results obtained as provisional. The author indeed claims no more. The research is more or less a side-line in his work, the chief aim of which is to determine how far the brain of the animal can replace the human brain "in numerous simple industrial operations". It is worth noting that the dog was found to be too neurotic a subject for the experiments and had to be given up.

The remaining paper is a rather long and somewhat technical one on "The Sensation of Recoil in Shooting, and its Relations to the Mechanical Values of the Recoil" by General Journée. The results of experiments carried out at the *Société Française des Munitions* with different types and weights of gun, and with different kinds of powder, are given and discussed. One not unexpected result obtained was the difference in the sensations reported by two practised riflemen under the same conditions. The paper has a somewhat specialised and limited appeal, but describes a very extensive piece of experimental research.

J. DREVER.

A Theory of Direct Realism and the Relation of Realism to Idealism. By J. E. TURNER, M.A., Ph. D. London: George Allen & Unwin, Ltd. (Library of Philosophy), 1925. Pp. 324. 12s. 6d.

In so far as this book may be said to present a continuous thesis, the plan of it appears to be somewhat as follows: Beginning with a consideration of the discussion between Profs. Stout and Alexander recently published in *MIND*, the author claims to evolve a theory of 'direct' realism (in matters of sense-perception). This occupies Chapters I.-VII. Thereafter (with certain explanations) we have, in the main, a discussion and a reasoned rejection of certain other theories of a realistic cast such as Kemp Smith's or Broad's (Chapters VIII.-XIV.). A section of four chapters (XV.-XVIII.) follows in which, speaking broadly, there is a free discussion of important questions, partly in epistemology partly in ontology. Lastly, (XIX.-XXII.), there is an attempt to show that the author's 'direct' realism is not only consistent with (Hegelian) idealism but demands it.

By 'direct' realism the author means that 'despite the complexity of perceptual activity, the mind or *ego* is thereby maintained in immediate contact and relation with the external material world as this actually exists' or that 'perception is in principle veridical,' and he goes on to say (p. 9) that he 'does not think this task has hitherto been attempted.' Coming from one who has acquainted himself so thoroughly as this author has with the whole range of British and American 'realism,' the novelty alleged in this claim seems altogether amazing. It is supported, however, by the further claim that the 'formative period of early

'consciousness' has been wrongfully neglected by all other realistic systems although not by this one. Quite possibly the former part of this statement is true, but, despite his claim, the author's own treatment of this important matter seems to me so slight as to be scarcely even perfunctory. If this is not the case, I can only say that Mr. Turner's idea of the meaning of 'formative periods' or of 'early consciousness' seems to be peculiar to himself. There is perhaps something more substantial in his third claim that his treatment of causality in connexion with his realistic theory is a new departure. To say, however, as this author does, that *all* the perplexities (of phenomenism, noumenalism, etc.) disappear 'when we regard the entire series of physical and physiological processes as determining, not the actual existence nor real character of the object of perception, but simply *perception itself* as being also a process, but of a higher order than these preliminary basal processes' (p. 160) seems to me an outrageous overstatement. It also appears to me to be flatly inconsistent with the author's eager readiness to employ the 'continuity of cerebral process' as an explanation of the similarity between after-image and object (p. 183) and of dozens of other such employments that he makes when it suits him.

The discussion of other realistic theories appears to me to be upon the whole a good and a useful piece of work, and the discussions of imagery, feeling, matter, space and time have also a certain value. It is more doubtful, however (as I think), whether the concluding part of the book has any considerable value. Certainly Hegel repeatedly repudiated subjectivism. So did Kant, although Hegel would not accept Kant's repudiation. If we grant, however, as I think we should, that Hegel's philosophy is *not* appreciably tainted by private mentalism, all we have to say is that Mr. Turner is one of many who have pointed this out. Very likely, it is true, there is room for many more (especially when, like him, they produce documentary evidence), and there are far too many loose 'realistic' arguments against Hegelianism which, even if they have a certain relevance to some of the things that some British 'Hegelians' have said, are manifestly *not* a fair comment upon Hegel. In the main, however, what has to be shown is that, according to Hegel, the place of Mind in the universe has no genuine analogy to the function of minds in mentalistic accounts of it, and if not, why not. Here, so far as I can see, Mr. Turner denies any analogy, but produces no reasons for the trenchancy of his denial. Thus he tells us, in the end, that 'mind is never a realm wholly separate from matter' (p. 313); yet he has told us on page 44 that the original meaning of physical or sensible existence is to be non-mental. (I admit I do not know what he means by 'mind,' for on page 163 he tells us that the *organism* becomes the experiencer, and on page 291 actually states that it is 'convenient' to use 'mind' to denote 'all phenomena and processes other than material'!) For the rest we have the trivial remark that mind and matter cannot be absolutely opposed because in our actual experience they are always found related (p. 292). Again, if anything seems plain in Chapter XV., it is that an image, being significant, *must* be different altogether from a sensed-content because the latter is non-significant; yet the ideal significance of the physical realm appears to be the final triumphant conclusion of this author's 'idealism,' buttressed (as it seems to me) by a grovelling idolatry of "Science" and by the incredibly naive statement that 'the later in time is universally the higher in organisation—a principle which is established by its very exceptions' (p. 292).

JOHN LAIRD.

Allgemeine Ontologie der Wirklichkeit. By GÜNTHER JACOBY. Vol. I. Halle, Max Niemeyer, 1925. Pp. 576. 11 Rentenmark (gebunden 13.50).

The ostensible purpose of this book is to examine the concept of *Wirklichkeit*, but the subject naturally tends to expand till it covers almost the whole of metaphysics. However, if this is a defect, it is mitigated by the fact that the book is a work of great ability, sound, suggestive and readable. The first part deals with the kind of reality possessed by the physical world. The author treats it from the standpoint¹ that physical objects are numerically identical with contents of our mind, but he later discards this standpoint as only practically and not theoretically workable, and it is in consequence not clear whether he means to retain his whole account of the physical world in spite of this change of position. The method of writing the first part of the book from a standpoint that he intended afterwards to reject is confusing, where owing to the length of the book and the number of subjects treated it was especially desirable to avoid confusion. In the account of the physical world I may mention two points of special interest. In the first place, the specific kind of reality the physical world has is distinguished from other kinds by the quality of *Naturgesetzlichkeit* and this quality is held to be implied in the existence of any reality given as external relatively to us, in *unserer Aussenwirklichkeit*. The argument seems to be that we can only know that something belongs to the physical world if we can connect it with other things by natural laws (*Naturgesetze*), e.g., this is the way of distinguishing external reality from dreams, and the truth of it is further shown by pointing out that a physical thing which produced no effects could not be perceived and would be in every way as good as nothing. Secondly, the treatment of the spaces of the different senses is very striking. The author denies any causal connexion between data of different senses and substitutes for it a correspondence,² which enables us either like the scientist to insert in the space of sight entities corresponding to the data of other senses, e.g., sound-waves, or like the plain man to put the data of other senses themselves into the space of sight without modification. Either method gives us for practical purposes a single space, but as far as perception goes the space for each sense is different.

The account of the self contains valuable sections on the questions of self-identity (*circ.* pp. 170 ff.) and the unconscious (pp. 205 ff.). For the author the unity of the self consists in (1) a unity of experience uniting everything that is contained in our consciousness at the same time, but not what is contained in it at different times, (2) as regards the identity of the self at different times the only connecting link he is able to give is causality, and further a causality without continuity or an *actio in distans* as regards time. The self, he contends, is identical only in the sense that the concept of any self as a whole must include all the states of that self at different times and is therefore self-identical independently of time like any other concept, not in the sense that there is something identical in the states of the self at different times. The discontinuity of consciousness and the impermanence of the elements included in it are strongly emphasised in contrast to the continuity of physical causation and the relative permanence of physical things, though it is pointed out that continuity of physical causation is only secured by the assumption

¹ *Der immanenzontologische Begriff und die Ueberschneidungslehre.*

² We must not call it a one-one correspondence, since there are gaps in the spaces of the different senses especially with a sense like hearing.

of entities which are not perceptible and are therefore not in the full sense physical: in its *prima facie* appearance the external world is by no means continuous, with its sharply defined "things." We may welcome this logical and clear statement even if we feel that more "self-identity" than this is required.

The author then discards the conception of physical objects as identical with elements actually contained in our consciousness and insists that some transcendence is necessary. A novel argument for this view, elaborated at considerable length, is that as regards the relation of body and mind we must adopt either some form of parallelism or some form of the interaction theory, and a diversity between the mental and the material elements is required by both alternatives. The change of position is so important as to call for more elaboration and readjustment than the author seems to realise. Also surely the situation required some explicit treatment of the specifically idealist position, which is sadly neglected.

The last part of the book deals with the nature of truth. His change of position does not prevent the author from holding that truth consists in identity between a concept and its object, the concept not being an actual part of the contents of our consciousness but just what we *mean* by our thought. The essence of truth is not to be found in independence, or universality, or necessity, because all these are properties relative to our consciousness, but in identity. Considering the diversity of subjects with which the book deals it is very unfortunate that it is not supplied with an index, especially in view of the considerable intrinsic value of the author's contribution to many vital problems of philosophy.

A. C. EWING.

Social and Political Ideas of some great thinkers of the Renaissance and the Reformation. A Series of Lectures delivered at King's College, University of London. Edited by Prof. F. J. C. HEARNshaw. With a Preface by Principal ERNEST BARKER. London: Harrap & Co. 216. 7s. 6d. net.

This is a series of studies by competent scholars. The editor contributes an Introduction on the Renaissance and the Reformation, and a Lecture on Machiavelli, Prof. E. F. Jacob writes on Nicolas of Cusa, Miss A. E. Levett on Sir John Fortescue, Dr. A. W. Reed on Sir Thomas More, Prof. J. A. K. Thomson on Erasmus, Prof. J. W. Allen on Luther, and Prof. W. R. Matthews on Calvin.

Prof. Jacob's contribution is the one that has most immediate interest for readers of MIND. It is a careful study of a writer whose work is only beginning to be appreciated. The chief stress is, of course, laid on his contributions to the study of social and political problems, but the general bearings of his philosophy are indicated and a useful bibliography is supplied. In furnishing these materials Prof. Jacob has rendered a very real service to the study of the history of philosophy. Most of the historians of philosophy have referred to the work of Nicolas in a very cursory fashion. Falckenberg, who gives him the first place in his *History of Modern Philosophy*, is the only one who seems to have had any adequate sense of his importance. Prof. Jacob calls attention to the fact that, in his doctrine of the unity of opposites, he may be said to have laid the foundations upon which the philosophy of Hegel was built. Hegel himself appears to have been hardly aware of this. It is perhaps a pity that, in giving an account of the doctrine of Nicolas, Prof. Jacob has thought it necessary to invert the commonly recognised dis-

tion between Understanding and Reason. He represents Nicolas as maintaining that the unity of opposites is beyond the grasp of Reason and can only be apprehended by the Understanding. No doubt it is tempting to translate *Ratio* Reason, but Falckenberg had already shown the example of calling it Understanding. *Intellectus* in its lower form should surely be called Reason: in its higher form the best name for it is probably Intellectual Intuition. The latter conception appears to contain an anticipation of the views of several modern philosophers, notably that of Bergson. The general doctrine of Relativity may also be said to have been anticipated by Nicolas. Such anticipations go far to justify Falckenberg in regarding him as the founder of Modern Philosophy; and it is to be hoped that Mr. Jacob will follow up his brief references by a more complete treatment.

In the meantime, it is mainly to the more social and political aspects of the work of Nicolas that he directs attention. The conception of the unity of opposites naturally led to an anticipation of the possibilities of political unity. Here he was perhaps wiser than Hegel. '*The coincidentia oppositorum*,' says Mr. Jacob, 'is the main preoccupation of this remarkable man's thought. It was unfolded in a subtle and profound system of philosophy, illustrated throughout by geometrical diagrams and elaborated with a mathematician's care; it was shot with a mystic's emotion and made high and holy with the devotion of a lofty spirit. . . . The constitutional doctrine which was its outcome is part of the legacy of the Middle Ages to the political thought of the West. . . . The forms of political organisation which it advocated soon died out of men's thoughts; but its spiritual core, consent and representation, compromise without extremities, unity through the conference, is being fought for still.'

The other Lectures in this volume, though less directly concerned with philosophical ideas, are hardly less interesting. The one on Erasmus struck me as a particularly good example of a clear and well balanced statement. Some of the Editor's closing sentences about Machiavelli are worth quoting. 'He ignored goodness in man just as he ignored gun-powder in war. Goodness and gun-powder! . . . In the art of war the development of firearms has swept the Machiavellian precepts into ridicule and oblivion. In the art of politics the conscience of mankind has repudiated the Machiavellian maxims, and the experience of the human race has demonstrated their folly. The records of history tend to show that Socrates and Plato were right when they said that in the long run the knave and the fool are one and the same.'

J. S. MACKENZIE.

The Neo-romantic Movement in Contemporary Philosophy. By SHISHIR KUMAR MAITRA, M.A., Ph.D. With a Foreword by LUDWIG STEIN, Berlin. The Book Company, Ltd., College Square, Calcutta. Pp. iv, 268.

Prof. Maitra starts with a general idea of the Neo-romantic movement in Philosophy and traces its manifestations in various forms—the Individualistic romanticism of Nietzsche, the Race-romanticism of Chamberlain, the Rhythmic romanticism of Keyserling, and the Poetico-religious romanticism of Dilthey—in the second, third, and fourth chapters of his work. In the other five chapters he deals with voluntarism, pragmatism, the philosophy of values, vitalism and energism, and the philosophy of Bergson. By romanticism Prof. Maitra understands a protest against the claims of reason to construct a philosophy by its own strength, and an

attempt to construct a philosophy of the real which comprehends the whole and is not one-sided in character like the rationalistic systems. All philosophy is in a sense romanticism and signifies a spiritual unrest, a longing for the totality of things unattainable by rationalistic systems aiming at asserting the supremacy of reason; and the romantic spirit which gives rise to such a philosophy refuses to be tied down to any logical scheme. It therefore prefers feeling to reason; for feeling alone has direct and constant access to the real, as it is always moving with the tide of life and the system of the world process. Professor Maitra admits that feeling divorced from reason is a weak principle, weaker than reason divorced from feeling, and therefore romanticism has failed to give us any positive construction in philosophy; but its chief merit consists in bringing into view the defects of reason. He also refers to the other type of romanticism which asserts the supremacy of will; but it also is as subjectivistic and individualistic and mystical as the feeling-romanticism. This emphasis of the personal element leads the romanticist away from a full view of reality from the point of view of totality, and lands him in the vagueness of a dreamy mysticism which obscures everything. Prof. Maitra thinks that "the real is the totality of all principles and not simply this or that isolated element." Philosophy is *Totalitätsdenken*, the reference of all things to the totality of relations. The romanticist has clearly failed to achieve this ideal and is as one-sided as a rationalist. Reason can never have access to the whole reality; but, though its logic is thus incomplete, there is always the hope of its being more and more complete as more of the real comes under the eye of reason, and reason though it may not ever know the whole of the real will gradually come to reveal it increasingly more and more. It is a pity that apart from the general criticism of romanticism Prof. Maitra does not give us separate criticisms of the various romanticist philosophers whose views he briefly describes. Prof. Maitra's exposition of these philosophers is exceedingly lucid and clear, concise and pointed. His book will be particularly useful for its exposition of the views of many contemporary German romanticist philosophers who are not well known among the philosophers of English-speaking countries. Though Prof. Maitra is fully aware of the failure of romanticism to solve the ultimate problem of philosophy, it must be said to his credit that he has shown great sympathy and tenderness in dealing with the views of the romanticist philosophers, and I was sometimes led to suspect that he was himself a romanticist at heart.

S. N. DASGUPTA.

Saint Jean de la Croix et le Problème de l'Expérience Mystique. By JEAN BARUZI. Paris: Félix Alcan, 1924. Pp. vii, 790. 40 francs net.

Judged by size alone Dr. Baruzi's volume might be called monumental. It is so, however, in more than bulk. It is a monument of patient, industrious, historical research. In addition to a couple of books on Leibniz, Dr. Baruzi has already published a previous volume translating some of the writings of the great Spanish Carmelite mystic. Now, after ten years of special research, he has issued what will undoubtedly be a standard authority on St. John of the Cross, and a valuable contribution to our understanding of what the author calls the problem of mystical experience.

The work embraces a comprehensive review of the sources, and an extensive list of authorities quoted, a list which covers all the principal literature of Mysticism, French, Spanish, English, and German. It affords a life of the saint which has been cleverly built up out of somewhat scattered materials a large number of which are unpublished. It includes an interesting account of his relationship to St. Theresa, whose disciple

he was. All this, however, is more or less incidental to the main theme of the book which is not merely concerned with the life of St. John, nor with the intricate task of the sources. It is to utilise the experience of St. John as an illustration of the psychological and metaphysical significance of mysticism. In order to do this it is necessary, of course, to place St. John amongst the higher type of mystics, if one may be allowed to be sufficiently "intellectualist" to call the conscious and rational type of mystics the higher. Dr. Baruzi argues, not unsuccessfully, that St. John belonged to this type, but that his unfamiliarity with intellectual tradition and vocabulary has concealed the true character of his mysticism, which was to find himself by losing himself, in other words to empty consciousness of all otherness in order to attain true self-knowledge. The difficulty of passing judgment upon such a contention is that those who have nothing like the acquaintance Dr. Baruzi possesses of the facts must hesitate to challenge his judgment. Yet at the same time the very fulness of a writer's knowledge is a frequent cause of his over-emphasising his subject. None the less it seems that Dr. Baruzi has made out his case. Anyhow he has contributed a very competent study of what might be called the metaphysical significance of mysticism, and in this respect his monograph possesses an interest even wider than that of its subject.

E. S. WATERHOUSE.

Grundlagen der Philosophie. By ÁKOS VON PAULER. Berlin and Leipzig: Walter de Gruyter & Co., 1925. Pp. x, 348.

This book (whose author is Professor of Philosophy in Buda-Pest) contains a sedate and placid presentation of rationalism over an enormous field which is partitioned into Logic, Ethics, Aesthetics, Metaphysics and Ideology. The partitioned provinces are themselves very tidily arranged, and the movement of the author's thought almost always seems to be effortless. This commendable quality, however, is not always achieved in the most commendable way. The author's facile acceptance of 'psychomorphic' idealism, for example, and his consequent rejection of Bodies and of Matter (except as phenomenal) is scarcely impressive; and his complaint that Causality tells us nothing of the Dasein of Substance is even less impressive, when we remember that he *defines* substance as "das selbsttätige Seiende."

The best parts of the book are the elaborate historical retrospects which conclude each section. Their range is truly enormous—indeed the author's Namenregister contains over a hundred names; yet it is not at all superficial; and if it is not always a first-hand account (as it appears to be) it is, in the rare exceptions, the very best of second-hand. A rather careful comparison between Plotinus and Husserl, to choose an instance, or a detailed account of Avicenna on the Categories, is as rare as it is refreshing; and it is pleasant to find the opinions of Augustine, Scotus Eriugena, Duns Scotus, Grosseteste, De Morgan and Bolzano treated as if they were upon the same level of vitality. The principal Hungarian thinker referred to appears to be Böhm; and the following remark may interest an English reader, "Nicht Sophokles ist der weiseste unter den Dichtern, auch nicht Molière, sondern Dickens" (p. 177).

The book is pleasantly written, but perhaps it is to be hoped that the terms "logisma" (concept), "usiological" (existential) or "hyparchological" (subsistential) will not find their way into English. *Per contra*, the term "autothetic" might find a useful place by the side of "analytic" and "synthetic."

JOHN LAIRD.

Platonism and its Influence. By A. E. TAYLOR. George G. Harrap & Co., Ltd., London, Calcutta, Sydney. Pp. ix, 153. 5s.

No living writer, probably, other than Professor Taylor, could have written so excellent an introduction as this to the meaning and history of Platonism. To the writing of this little book there has gone not only a deep insight into and a high enthusiasm for Plato's teaching, but an encyclopædic knowledge of later philosophy and theology. In successive chapters Professor Taylor treats of the Platonic Tradition, the Principles of Science, the Rule of Life, and Plato the Theologian. The details of Platonic doctrine cannot of course be given in a book of such small space, but the writer seizes with the greatest skill the most salient features of Plato's meaning, and shows with no less success how time after time philosophy and theology have had new life breathed into them by the revival of some element of Platonism. The influence of Plato on pure letters has had, from considerations of space, to be omitted, but every other side of his influence receives due recognition. Professor Taylor's own view of the relations of Plato to Socrates is not obtruded, and indeed much is treated as 'Platonism' which on that view is more properly 'Socratism'. But it is at any rate what the world has agreed to call Platonism, and what has in any case only reached the world through Plato's golden pages. Professor Taylor shows, I think, too much of a tendency to treat the teaching of Aristotle as a watered down or vulgarised Platonism, and I believe that a truer view would recognise the transcendent merit and the great originality of both thinkers. But that is the only general criticism I have to make on what is, on its own subject, a delightful and valuable book.

W. D. ROSS.

Comment Diagnostiquer les Aptitudes chez les Écoliers. By DR. ED. CLAPARÈDE. Ernest Flammarion, Paris, pp. 297.

This is a comprehensive though brief study of aims and methods which have been adopted in the testing of general intelligence and special aptitudes. The author approaches the matter in a broad way, considering not only, for example, the method of correlation of tests and of age standardisation, but the evaluation of tests by the method of introspective analysis and the study of the general psychology of individuals and of their mental development. The technique and the results of many kinds of tests receive attention, and one notes with satisfaction the exceptional knowledge shown by a continental writer of the work of English and American contemporaries. Dr. Claparède is always clear, and reveals throughout a balanced judgment and a sense of proportion. Such a large topic can, of course, hardly be dealt with exhaustively in a book of three hundred small pages, but an adequate index, which is unfortunately missing, would have revealed the very wide range of his treatment, and the large number of workers of whose researches he has given some account.

"Tests D'Aptitudes" occupy about one-third of the book, both methods and results being briefly treated. One turns with interest to Dr. Claparède's treatment of tests of moral judgment. After examples of tests involving the classification, according to degrees of wickedness, of a series of lies, a series of cruel acts, and a series of thefts, tests quite useful within their limits, one meets the common sense observation that to perceive the degree of badness of an action does not imply incapability of performing it.

To sum up, we can heartily recommend the book as a useful introductory study of the subject.

C. W. V.

Psychology and the Sciences. Edited by WILLIAM BROWN, M.D., D.Sc., M.R.C.P. London: A. & C. Black, 1924. Pp. 184.

A series of lectures was delivered at Oxford in the Michaelmas term of 1923, designed to answer the question: What are the views of specialists in allied sciences as to the value of recent advances in Psychology? Prof. J. S. Haldane opens, on the relations of Psychology and Biology. The line of thought is that of his contribution to the symposium at Durham in the same year; and one may anticipate that it will be more exhaustively expounded in his forthcoming Gifford Lectures. Dr. R. R. Marett deals with the anthropological bearings of Psychology; whilst Dr. Schiller endeavours to "improve the relations" between Psychology and Logic. Ethics and Theology are discussed in the light of the central topic by Principal L. P. Jacks and the Rev. A. E. J. Rawlinson respectively. The application of Psychology to Education is treated by Dr. M. W. Keatings, and its influence upon Medicine by the Editor. The concluding lecture deals with Psychical Research. Space does not allow all the points of interest in these lectures to be dealt with here, and it would be invidious to select any individual contribution for special consideration. Each succeeds in raising questions of importance. The interest of this volume does not lie, however, only in its presentation of modern psychology; it indicates clearly certain trends in scientific thought in general. A critical attitude towards "nineteenth century science" pervades almost all the lectures, and the criticism strikes at the logical as well as the material content of this structure. Curiously, the defence of the more purely rational position is left to exponents of systems of thought until recently assumed to be hostile to scientific method in general. It is Principal Jacks who points out that some of the methodological substitutes employed are in danger of undermining the very science which proposes them. A similar criticism of the introduction of certain psychological theories into scientific method is advanced in the discussion of the bearings of psychological doctrine upon Theology.

C. A. M.

Skill in Work and Play. By T. H. PEAR, M.A., B.Sc. London: Methuen & Co., 1924. Pp. 107.

The discussion, in connexion with the more familiar matters of Industrial Psychology, of the conditions of physical skill in sports and recreations, is characteristic of Prof. Pear's freshness of mind in treating topics beginning to wear threadbare. This note of originality occurs throughout in well-chosen illustrations and interesting suggestions on various matters thrown out by the way.

The circumstances of this book's original composition—it appears to be based on a series of non-technical lectures—no doubt prevented the author from following out certain trains of thought as far as he would have wished. In particular much more might have been said about the proposed language for describing kinaesthetic sensation and the notation for simple movements. To appreciate the full significance of these interesting suggestions the reader is compelled to refer to the author's earlier writings. This one is, professedly, only a slight volume, intended "to open a few discussions which, at the time of writing, seemed rather unlikely to open themselves"; but it might easily be the germ—in its own particular field—of much useful constructive work.

C. A. M.

Agostino, dal "Contra Academicus" al "de vera religione." By A. Guzzo. Florence, n.d. [1925]. Pp. xi, 158.

Verità e Realtà. Apologia dell' Idealismo. By A. Guzzo. Turin, n.d. [1925]. Pp. v, 149.

The former of these little books describes the gradual development of St. Augustine's thought from the time of his conversion to the composition of the *de vera religione* in 390, with careful analyses of the various Augustinian works which belong to this period. It may be warmly recommended to all students of the greatest thinker of the early Western Church. The second booklet is made up of two essays explaining and defending the type of "idealism" best known in this country from the writings of Gentile. To those who, like the writer of this note, are among the adherents of the Greek philosophical tradition that the primary *cognitum* is always a *res*, the explanation unfortunately seems obscure. We do not recognise ourselves in the "realist" set up by Italian idealists for demolition, and when we wonder whether we *ought* to recognise ourselves in him, we are baffled by the imaginative rhetoric of these eloquent apologists. I have often wished that some member of the movement would attempt a restatement of its position with scholastic precision, and in the Latin language.

A. E. T.

Notes d'épistémologie thomiste. By L. NOEL. Louvain and Paris, 1925. Pp. vii, 242.

A collection of brief and lucidly written essays, several of which have already appeared in the Louvain *Revue de philosophie néo-scholastique*. M. Noel's main object is to explain precisely what is and what is not implied in the Thomist "direct realism," and to defend it against all theories of "indirect realism." The point to be established is that in "simple apprehension" we have a direct confrontation of the knower with a real object other than the knowing mind, and that but for this direct, though partial, contact with the "real" in simple apprehension, there could be no escape from the reduction of all supposed knowledge to the insignificant train of "subjective processes." In particular, the notion (often adopted by the average man of science), that we could argue to a real world from data which are "subjective states" by the principle of causality is thoroughly illogical. These points, to my mind indispensable to any rational theory of knowledge, are convincingly argued. In many ways the most "actual" essay of the Volume for most British readers will be that on *Le problème kantien* of which an analysis has already been given in *MIND*, N.S., 136, p. 518-519. The author subjoins a study of the "thing in itself," tracing its history from the *Dissertation* of 1770 to the second edition of the *Kritik* and drawing the interesting conclusion that Kant is no precursor of "idealism" but, at heart, an "indirect realist." The inconsistencies of his teaching about the "thing-in-itself" are inherent in the indirect realism to which he was always faithful and which he has pushed to its full consequences.

A. E. T.

Received also :—

- T. Ziehen, *Vorlesungen über Ästhetik*, I. and II. Teil, Halle am Saale, M. Niemeyer, 1925, pp. 300 and 420, M. 22.
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VII.—PHILOSOPHICAL PERIODICALS.

BRITISH JOURNAL OF PSYCHOLOGY. xv., 3 (Jan., 1925). **Professor C. Spearman**, in an article on "The New Psychology of Shape," discusses the fundamental ideas of two (largely German) schools of thought, (a) that which divides the perception of shape into two steps, sensations and a process superimposed on them, and (b) that which regards the "shape" as consisting of one indivisible whole or "configuration". The author dubs the schools binarians and unitarians respectively. He criticises the tendency of the binarians to base their position on physiological grounds and for their needless introduction of metaphysics, and both schools for errors in the faculty psychology. Finally the author suggests a resolving of the dispute by the use of his fundamental laws of cognition, the natural evoking, by all experience, of awareness of its own nature and the tendency for the apprehension of two items to evoke an awareness of relation between them. "Economy in Motor Learning," by **M. Gopalaswami**. The mirror-drawing experiment was done by four different methods; (a) the whole method, the whole figure being traced three times a day till done efficiently; (b) the pure part method, each quarter of the figure being learned separately and then the four learnt in combination; (c) the progressive part method, the first quarter was learnt, then the second, then the two together, then the third, then the three together, and so on; (d) the two part method, the two halves being learnt separately and then combined. The results showed: (1) a considerable superiority of the progressive method (distributed); (2) greatest inferiority of the whole method, and then of the pure part method (massed); (3) tendency of the pure part method (massed) to increase the scatter, as if the method were advantageous for some subjects but disadvantageous for others. The repeating of a task is said to be "massed" when no interval is given between the repetitions; it is "distributed" when, on the contrary, intervals do occur. The general conclusion for neo-genetic work is that massing is advantageous up to the limits where fatigue, specific or general, begins to be seriously felt. If the parts are such that relations learned in one can be applied to another, then general massing is better than purely part massing. **E. Farmer** and **E. G. Chambers**, in "Psycho-Galvanic Reflex in Psychological Experiments," give a résumé of work done by others on the psycho-galvanic reflex, and indicate the possibility of physical and physiological complications. They conclude that there is overwhelming evidence that changes in affective tone are followed by galvanic deflexions indicating a rise in conductivity. We cannot say that every rise in conductivity is the result of an emotional change, since other causes can have the same effect; yet we are safe in assuming that an affective tone above a certain level has a psycho-galvanic reflex corresponding to it, provided that physiological factors are not unduly disturbing the operation of the phenomenon. They conclude also that the psycho-galvanic reflex is not a reliable means for measuring differences between individuals, as the factors of error are so many. This does not, however, preclude the use of the psycho-galvanic reflex as a means of group differentiation. If the difference between two groups as measured by the psycho-galvanic reflex,

is greater than any difference which could arise by any mere chance, then we can assume that the difference is significant. "An Experimental Investigation of the Psychology of Moral Judgment," by **G. A. Johnston**, records an investigation among 329 Glasgow Training College students as to (1) the agreement of moral judgments, (2) the question whether they are based on principles explicitly or implicitly used, (3) whether the standard is reason or feeling, and (4) the place of reflexion in moral judgment. Six problems were presented, e.g., the actions of Antigone, and of Captain Oates (in Scott's polar expedition), the case of the starving man who steals bread, and the doctor who lies to save his patient. A "snap" judgment, yes or no, was obtained on each problem after half a minute; a reasoned judgment ten minutes later. Sixteen per cent. of the snap judgments were changed on reflexion, usually from "wrong" to "right," confirming the findings on other grounds that judgment was usually based on reason and not on feeling. While snap judgments imply conventional standards, reflexion showed these conventional standards to be inadequate. The disagreement in judgments was remarkable, some classes of students being about equally divided in reference to certain of their problems. The answers characteristic of one or two classes suggest some influence of a "group-spirit". There was little evidence of the recognition of general moral principles except that an action is right if its motive is right. In an article on "The Present Status of the Concepts of Nervous and Mental Energy," **R. D. Gillespie**, after a résumé, with comments, on various views about energy manifestation in peripheral nerves and the energetics of the nervous system, discusses the use of the energy concept in psychology. The argument (quoting Myers) tends in favour of a "special kind of energy unknown to us," a neuro-psychic energy. So obvious is the failure to reduce all mental phenomena to purely physico-chemical terms (although further elucidation in this direction is foreshadowed by the work outlined above) that Ostwald was forced to postulate the existence of a special variety of energy, namely psychical energy, to account for them. It is important to remember that this difficulty of the limitation of physico-chemical explanation is not confined to problems of nervous and mental energy, but extends to the field of all biology (with which some would now consider "mind," in its widest sense, as co-extensive). Another article in this number is 'The Effect of Binaural Phase Differences on the Localisation of Tones at Various Frequencies,' by **H. Banister**.

xv., 4 (April, 1925). **J. C. Flugel** in an article on "Feeling and Emotion in Everyday Life," gives an account of the records kept by nine trained observers as to their feelings of pleasure and "unpleasure" and their causes at frequent intervals over a period of thirty days, marks being given to indicate also the intensity of the feelings. Reliability was estimated by the correlation of the records for odd and even numbered days over the whole period. The results show in every case a predominance of pleasure over unpleasure, the degree of predominance varying, however, considerably from one subject to another. Individuals also varied greatly as to the main sources of pleasure and unpleasure. Thus in one, emotions directly or indirectly concerned with action seem to be prominent. Another, easily the happiest of all the subjects, finds the chief source of happiness in "pleasant sensations". Another gives the chief places to "contentment or discontent". Even among these intellectual persons the pleasures of food are put among the six most important sources of everyday enjoyment, but "interest" comes first of all. Unpleasant sensations are the chief source of displeasure, being somewhat weightier even than anxiety and worry, anger, fatigue, or boredom. There is some evidence to suggest that those who tend to experience most the extreme degrees of feeling are on the whole less happy than those whose feelings are usually less

intense. "The Co-existence and Localisation of Feelings," by **Paul Thomas Young**. This paper offers a criticism of Wohlgenuth's findings on the basis of his experiments that "two or more feeling-elements may co-exist in consciousness; they may be like or they may be unlike," and that "Feeling elements can often be localised" (*Brit. Journ. Psych. Monographs*, VI.). It is claimed that an analysis of Wohlgenuth's study shows that so-called co-existing feelings depend upon an attitude of observation which may be characterised by (1) reference of feeling to object, (2) a tendency to localise feeling at the place of the object, (3) a tendency to judge the object and to compare objects in terms of pleasantness and unpleasantness. This attitude of observation gives rise to a type of report which is ambiguous as to what was felt and as to the temporal relations of affective experience. It also favours a confusion between statement of meaning (of pleasantness or unpleasantness) and direct report of affective experience. **T. H. Pear**, in an article on the "Privileges and Limitations of Visual Imagery," discusses the question as to whether the man whose thinking takes place largely through visual imagery is more hampered than the one whose thinking depends entirely upon words (the verbalist). He argues that the anticipation of some experiences is dependent on imagery, and that some "visiles" have not fully realised the possibilities of using their imagery. Words may suffer as much from their common interchange as images do from the fact that they are private property. And words may equally provide a "cage" or "bondage" for the verbaliser. The possibility of a "visual logic" is suggested. **Frances Gaw**, "A Study of Performance Tests". Fourteen Performance Tests (*e.g.*, Picture Completion, Cube Construction, Dearborn Formboard, Porteus Maze, Cube Imitation, Substitution, Healy Puzzle, Goddard Formboard, etc.) were applied to elementary school children and to canal-boat children, and norms were obtained from the form. Various general intelligence tests were also applied. The results suggest that, with three or four possible exceptions, the performance tests used are to a large extent, but in differing degrees, tests of some central mental capacity. There is presumptive evidence that the central factor measured by these tests is largely, if not wholly, general intelligence. The tests which correlate best with intelligence as measured by the Binet scale, are much the same as those which correlate well with the central "performance" capacity. Owing to the presence of specific factors, linguistic and non-linguistic performance tests supplement each other in very valuable and necessary ways. And it appears that with subjects from so very limited an environment as that of the canal-boat children, performance tests furnish a better means of judging intelligence than do the Binet tests. The average degree of inter-correlation between these non-linguistic tests is somewhat lower than that obtained in a series of tests of a linguistic type. It is, therefore, even more necessary with performance tests to use not one, or even two or three tests, but a number. There appear to be among individual children, specific tendencies or types, some subjects doing better throughout in linguistic tests, and others in non-linguistic. There are, at any rate in some of the performance tests, very well-marked sex differences. Generally speaking, the boys seem to be better than the girls, particularly in tests requiring perception of form. **H. M. Vernon**, "Can Laboratory Experiments on Output Throw Light on Problems of Industrial Fatigue?" This paper gives a survey of a number of investigations bearing on the problem set, and an account of some new tests; the writer concludes that in order to obtain information which can be directly applied to industrial problems, it is necessary to collect this information strictly under industrial conditions. An incidental conclusion based on certain of the tests is that when industrial

workers are engaged on monotonous repetition work involving quick movements, their mental processes are slowed and dulled. Hence they are not so liable to think of the monotony of their labour or to magnify their grievances as they would do when they are employed on monotonous work involving only slow and occasional movements, e.g., those that are often required in controlling semi-automatic machinery. "Mental Tests for University Students," by **Agnes L. Rogers**. The Thorndike tests for adults were applied to 254 women students at the entrance to their college careers, and their performance at graduation, four years later, noted. The correlation of one year's marks (in academic subjects) with those of any other year was never higher than .75. The correlation of tests, High School records, or entrance exams. with academic results showed that (1) the Thorndike examination could in two hours forecast success with University work as accurately as High School records covering four years and as well as college entrance examinations occupying twelve hours; and (2) the examination could be used best as a supplement to existing methods of admission, particularly to aid decision in doubtful cases. In individual cases, taken alone, it would have proved unreliable.

JOURNAL OF PHILOSOPHY. xxii, (1925), 11. **R. M. Ogden**. 'Crossing the Rubicon between Mechanism and Life.' [Holds that "the Rubicon which has been assumed to separate matter and life now appears as a figment of man's imagination arising out of the contrast between the exact methods of quantitative analysis and the practical pursuit of ends," and that "mental phenomena are as objective as physical". Proposes the name 'integration' for the factor common to psychical and physical.] **C. O. Weber**. 'Scientific Method and Moral Concepts.' [Suggests that "feeling and volition are as truly sources of comprehension as the intellect," and illustrates the mechanistic observer's inability to predict moral action exactly.] xxii, 12. **G. P. Conger**. 'The Doctrine of Levels.' [Taking 'level' as "a class of structures or processes distinguishable . . . as being higher or lower," the paper asks (1) what is a metaphysical level, (2) what levels does the world exhibit, (3) how do later develop from earlier levels, (4) what are the philosophic consequences of the doctrine?] **D. Drake**. 'Mr. Broad's Questions Concerning Critical Realism.' [Endeavours to answer the questions put by Broad in the Aristotelian Society's supplementary volume, 1924.] xxii, 13. **M. T. McClure**. 'Data and Meaning in Cognition.' [Data are "not things known but things we know with," they are "concepts of explanation, but not things explained". Only so can we avoid the 'inductive leap' of thought, which makes 'induction a mystery, deduction a guess and truth an accident,' or else reduces induction to tautology by identifying data and meaning.] **D. S. Robinson**. 'The Logical Significance of Rediscovered Knowledge.' [Thinks that to say a lost piece of knowledge has been rediscovered is only intelligible as a reference to a "supertemporal universal order-system".] xxii, 14. **T. V. Smith**. 'Professional Work as an Ethical Norm.' [Thinks professionalism is "the best available approach to the supreme ethical task of the hour—the humanisation of work".] **S. P. Lamprecht**. Meeting of the Western Division of the American Philosophical Association, at Urbana, Ill., in April, 1925. **R. M. Blake**. 'Professor Singer's Philosophy of Science.' [A review of his *Mind as Behaviour*.] xxii, 15. **H. E. Cory**. 'The Interactions of Beauty and Truth.' [The discussion of their relations suffers from the writer's taking both beauty and truth in their abstract generality and omitting to explain what he means by either: incidentally, however, he discovers "the ancient paradox of Epaminondas the Cretan" (!)] **F. J. E. Woodbridge**. 'Behaviour.'

[Argues forcibly that "behavioristic distinctions are teleological distinctions," that "we understand what things are only in terms of what they do," that "behaviour is a teleological matter implying a natural teleology," that "the relativity of ends is confirmation of teleology and not its undoing," and that "natural teleology constitutes the intelligibility of nature." But here again a little concrete illustration would render this paper far more convincing.] xxii., 16. **E. A. Singer, Jr.** 'On Spontaneity.' [The whole connotation of *life* lies in its purpose—*self-maintenance*—which is accomplished by 'sensibility and spontaneity,' otherwise knowing and willing. The teleological character of mind is compatible with 'the mechanical postulate,' that all events shall be calculable, if it is admitted that spontaneous acts are statistically calculable. This we assume, and if our calculation fails, we merely say that we had not sufficiently grasped the nature of the being that disappointed us. Thus sensibility and spontaneity may be "presented in terms of virtual behaviour. Virtual behaviour is a calculus of expectancy, based on data experimentally collected, and on a formula derived from the meaning of probability."] **B. I. Gilman.** 'Deity the Implication of Humanity.' [Argues that "I am under no logical compulsion to believe in the real existence of either Humanity or Deity. If, nevertheless, I do believe in the real existence of Humanity, I am thereby logically compelled to believe in the real existence of Deity."] xxii., 17. **E. T. Bell.** 'Mathematics and Credulity.' [An amusing paper, by a mathematician, deriding the absolute certainty claimed for mathematics by enthusiasts, and showing that its 'self-evident' principles analyse either into definitions or beginnings of the question.] **A. C. Benjamin.** 'Classification and Division.' [Distinguishes three senses of each, (1) classification of the individual, of species in a genus—or 'colligation,' and as the correlative of division in its three meanings, (2) 'particularisation,' division proper, and 'analysis'.] xxii., 18. **E. Wind.** 'Contemporary German Philosophy.' [Gives a clear account of Cassirer as representative of the Neo-Kantians, and of Rickert's theory of values.] **H. R. Smart.** 'The Factual Basis of Mr. Johnson's Logic.' [Deplores his "founding logic upon psychology," regards it as "an unwarranted psychological fiction" that "separateness must be prior to comparison," and concludes that the superstructure cannot be trusted "given this so hopelessly weak foundation."] xxii., 19. **L. A. Reid.** 'Moral Intuitionism, Feeling, and Reason.' [Distinguishes between an 'objective' intuitionism which holds that acts can be right and wrong *per se* and irrespective of consequences and a 'subjective,' which holds that their rightness and wrongness can be perceived immediately, denies that the 'sentimental' intuitionists appealed to mere feeling, and declares 'intuitions of Reason' to be self-contradictory, because 'reason' should involve 'reasoning'.] **E. Wind.** 'Contemporary German Philosophy, II.' [Talks about Max Weber, Simmel, Stephan George, Husserl, and Nicolai Hartmann, but does not in the end convince one that contemporary German philosophy has anything much to say, or knows how to say it.] xxii., 20. **A. Hall.** 'The Problem of Reality.' [After discussing Materialism, Idealism, Dualism, New Realism, Pragmatism, reaches the conclusion that "reality is not justly explained by a monistic philosophy. The world in its ontological and in its historical aspects must be conceived of in a pluralistic way."] **H. G. Townsend.** 'The Obscurantism of Science.' [Science serves us not only materially, but also spiritually, by "holding a magic veil between us and the terrifying chaos of experience". But "the systematic application of any scientific category to all phases of experience leads inevitably to a kind of myopia," and this overlooks the real complexity of things. The real reply of science to this charge is to admit that it "works with an

artificially simplified universe," and that "for certain purposes it is treating the world as if it were thus and so". If it does not explain this sufficiently, "its own obscurantism will beget a still more vicious obscurantism among those who do not understand its method".] xxii., 21. **L. E. Akeley.** 'The Problem of the Specious Present and Physical Time.' [Argues that specious presents are relative to modes of measurement.] **E. F. Carritt.** 'Ethics in Philosophical Education.' [Declares that "no philosophy is practical," and that "all that is valuable in ethics is formal" and expresses surprise that "members of my class actually approached me, as if I were a father-confessor, for the solution of special problems in conduct!"] xxii., 22. **E. A. Singer, Jr.** 'George Stuart Fullerton.' [Personal reminiscences.] **J. Dewey.** 'The Naturalistic Theory of Perception by the Senses.' [Argues against the 'epistemological' theory of sense-perception that "perception does not affect or infect the nature of the qualities perceived, although sense-organs and their structural connections, which are the means of perceiving, do affect the properties of the thing produced. But there is nothing unique or peculiar about this fact." For the epistemological theory, on the other hand, "sense-perception means a unique kind of perception, and sense quality means a kind of quality so distinctive that it may be called psychical or mental". The reasons for this belief are examined and shown not to be cogent.] xxii., 23. **F. H. Hankins.** 'Individual Freedom with Some Sociological Implications of Determinism.' [A complacent rehearsal of the ordinary arguments for determinism, with attacks on MacDougall.] **J. R. Geiger.** 'Concerning the "Good Man" and the Moral Standard.' ["The greatest good for the greatest number is, for practical purposes, the good as foreseen and appreciated when looking through the eyes and relying on the intuitions of one's best self."]

REVUE DE MÉTAPHYSIQUE ET DE MORALE. 32^e Année, No. 1 (Janvier-Mars, 1925). **M. Fréchet.** *L'Analyse générale et les ensembles abstraits.* [This article will interest students of philosophy who are capable of following, from a logical point of view, recent developments in mathematics. It summarises the work done, by the author and other mathematicians, on the elaboration of a generalised concept of abstract sets of entities, as the foundation of the Theory of General Analysis.] **Émile Boutroux.** *Travaux d'École Normale* (fin.). [A further instalment of essays written by Émile Boutroux as a student at the *École Normale*, with marginal criticisms by his teacher, Jules Lachelier. The essays deal with: (1) The possibility of perceiving distance by sight; (2) Final causes; (3) Are mathematical demonstrations syllogisms? On this last question, Boutroux comes to the conclusion that the connexions between major and middle, and minor and middle, terms in the syllogism always rest on experience, whereas mathematical reasoning proceeds by pure thought from identity to identity, and thus is the nearest human approach to the "immediate and simultaneous comprehension of all things by the Divine Understanding".] **C. de Waard.** *Les objections de Pierre Petit contre le "Discours" et les "Essais" de Descartes.* [This article prints for the first time a manuscript discovered in the *Bibliothèque Nationale* among the correspondence of Mersenne, which has been identified as a portion (the opening pages being lost) of Petit Pierre's criticisms of Descartes' theories. M. de Waard prefaces the manuscript with an account of Petit Pierre and of his relations to Descartes.] **E. Bréhier.** *La pensée grecque, d'après M. Léon Robin.* [A critical review of M. Robin's book on *La Pensée grecque et les origines de l'esprit scientifique.*] **M. Foucault.** *Plaidoyer pour la psychologie scolaire.* [The author replies to criticisms directed against his book on *Observations et Expériences de Psychologie scolaire.* He defends the

use of psychological observations and tests of school-children.] **C. Bouglé.** *Autour de la philosophie du travail. Le testament de Gabriel Séailles.* [Starting with a review of the main principles of Séailles' *Philosophie du travail*, the author applies these principles to the present attitude of the working classes towards their part in industrial production. He concludes that the "humanisation" and "democratisation" of industry imply that the worker's interest is not restricted to securing an adequate wage, but that he wants a measure of autonomy, an opportunity for creativeness, an outlet for the moral qualities of disinterested devotion, even of heroic self-sacrifice. Even a socialistically organised society cannot get on without the spirit of duty.] **E. Leroux.** *Souvenirs du Congrès de Naples.* **J. Jacob.** *A propos de la morale de M. Loisy.* [A note reasserting, as against the second edition of M. Loisy's *La Religion*, the author's criticisms formulated in an article in the *Revue de M. et de M.*, July, 1924.] New Books. Periodicals. Obituaries: F. H. Bradley; J. E. Creighton.

32^e Année, No. 2 (Avril-Juin, 1925). **L. Brunschvicg.** *Vie intérieure et vie spirituelle.* [An address delivered before the 5th International Congress of Philosophy, at Naples, in May, 1925. Its main point, which is rather obscured by an exasperating allusiveness of treatment, appears to be that the cultivation of, and absorption in, one's inner life is not necessarily identical with the cultivation of a spiritual life. For, interest in the inner life may be nothing more than a psychological interest in one's own feelings, emotions, imaginings, whereas a truly spiritual life consists in communion with others and in responding to universal values.]

P. Lévy. *Les lois de probabilité dans les ensembles abstraits.* [The author begins by recalling the difference between two kinds of laws of probability, viz. (1) laws for the choice among elements which are either finite in number or which constitute an enumerable infinity—these laws are *discontinuous*; (2) laws for elements in the form of points chosen at random on a line, on a surface, or in a volume, and grouped into infinite wholes—these laws are *continuous*. It is possible to consider these two kinds of laws as special cases of a single, more general, type of law. And if this is possible, may it not also be possible that the study of sets of elements of even greater generality may lead to yet other types of laws of probability? The author reaches the conclusion that this is not possible, but claims that the argument, in spite of its negative result, is nevertheless of great interest. The details, however, are too technical for the present reviewer to summarise them adequately.]

G. Marcel. *Existence et Objectivité.* [The author starts from the assertion that modern metaphysicians have been more interested in the *nature* of the object world than in its *existence*. To this neglect of the aspect of existence he traces a number of familiar fallacies, e.g., the fallacy of treating existence as a concept and a predicate; the fallacy of treating sensations as effects, or "messages," of an unknown *X*; the fallacy of treating the body as an "instrument" of the mind, or self. The author's own view is that existence—"the experience, confused and *globale*, of the universe as existing"—is one of the immediate data of philosophy which can never be treated as a problem, because it is the basis on which the mind must take its stand in considering any problem at all.]

M. Déat. *Kant et le problème des valeurs.* [Shows how the modern "philosophy of value" has its roots in Kant's distinction between theoretical and practical reason; explains the distinction as conceived by Kant; points out the difficulties and contradictions which beset it; and ends by suggesting that these difficulties can be overcome only by a re-integration of the factors which Kant's analysis divorces. The author finds approaches towards such a re-integration in the theories of J. M. Baldwin, L. Brunschvicg, and E. Durkheim.] **V. Jankelevitch.** *Georg Simmel, philosophe de la vie.* [The first instalment

of a long account of Georg Simmel's philosophy, in the form of a review of Albert Mamelet's *Le Relativisme philosophique de Georg Simmel*.] **G. Morin.** *La décadence de l'autorité de la loi.* [Points out that, under the old concept of sovereignty, which even the French Revolution did not challenge, but which it merely transferred from the monarch to the people, there is only one source of law, viz., the will of the sovereign people expressed through parliament; and that this law is to be applied, but not altered, by the judges. At the present day, on the other hand, sovereignty is being whittled down and dissolved, both externally [*cf.* the League of Nations] and internally. The chief sources of the internal diminution of sovereignty are (1) the growth of extra-parliamentary legislation by various social and professional bodies; (2) the modifications of the law by judicial interpretation.] New Books. Periodicals. Obituaries: J. McT. E. McTaggart; James Ward.

INTERNATIONAL JOURNAL OF ETHICS. xxxv., 1 (October, 1924). **Rupert C. Lodge.** 'The Platonic Value Scale.' [Investigates the grounds on which wealth, muscular strength, good looks, bodily health, courage, justice, temperance and wisdom are assigned their position in the ascending value scale, the principle governing the composition of the scale, and the meaning of such a scale for Platonism. Traces the transition from the mechanical—wealth, muscular strength, etc.—whose absence would be a nuisance but whose presence does little to stimulate their pursuit by spiritual beings, through those partly mechanical but able to contribute directly to spiritual life—courage, justice and temperance—to wisdom which, resting partly on curiosity, contains all the other spiritual excellencies fusing them into the ideal life. Suggests that for the man of insight there would appear to be no value scale since value belongs to the idea of good and this is one principle, but that the scale is valuable as expressing the degree of availability of the various factors as means for the reception and expression of values whose source is the idea of good.] **Bruce W. Brotherson.** 'Society; An Original Fact.' [Opposing the view that society is a construct, develops some implications of the view that it is an original fact, mainly in relation to the problem of moral evil; maintains that the universal bias to evil in human nature, illustrated by reference to the Greek dramatists, modern satirists, and Thomas Hardy, is a disordered condition in society in which the inner being of each individual is implicated, coercing him and preventing moral ability in him; develops the view that while originally no individual was conscious of himself as an individual and the early growth of individual consciousness was controlled by a growing social conscience, evil was introduced when the normal social conscience was broken by an individual leader; briefly suggests the bearing of this on problems of the origin of religion, conversion, and objective knowledge in religion.] **W. B. Graves.** 'Codes of Ethics for Business and Commercial Organisations.' [Gives examples of various codes, estimates their values, and suggests that they can be made to contribute much to the cause of truth and honour in business relationships.] **Karl F. Geiser.** 'America and World Co-operation.' [Objects to the League of Nations as now constituted on the ground that the Treaty of Versailles, of which the Covenant is a part, is the law of Europe and runs counter to the economic interests of European countries; rejects the view that order is peace; develops the view that the most important step towards American co-operation is the formulation and acceptance of principles applicable to the conduct of foreign affairs, based upon experience and international relations as they have developed; that neither law nor mechanism can reverse economic currents between states or change human nature; and that any effective policy of co-operation should be directed to-

wards the removal of restrictions between states, developing thereby the spirit of goodwill and a sense of justice.] **Che Fung Lui.** 'The Ethical Implications of Moh Tih's Philosophy.' [Surveys the period, about 500 B.C., when Moh Tih, one of the most brilliant souls of China, lived, philosophised, and founded the religion of Mohism, with its principles of Love all, Non attack, Thrift, and Condemning of music; contrasts Moh Tih with Confucius and Lao Tze; explains the meaning of the principles, and suggests that the political organisation of the Soviet government has most nearly realised Moh Tih's theory.] **C. Delisle Burns.** 'The Old Religion and the New.' [Combats the view that religion is obsolete; maintains that it is an element in the living experience of civilised man and is required to explain why men built temples rather than houses, composed requiems rather than drunken songs, discussed ultimate problems of life and death rather than contrivances for living; regards Christianity, Mohammedanism, and Buddhism as old religions, contrasts them with new religion whose dominant belief is confidence in reason independent of sacred rites or unexamined rules; traces the connexion between it and Christianity, and holds that the starting point of the new religion is not tradition but immediate experience; illustrates by reference to modern poetry, and concludes that the justification of the new religion will be, not in the logical proof that the old is mistaken nor in the excellence of science or art, but in the lives of its adherents.]

VIII.—NOTES.

DR. McTAGGART AND "IDEALISM".

THE reviewer ("L. S. S.") of my little book on *Idealism*, in the October issue of *MIND* (N.S. vol. xxxiv., No. 136, p. 513) charges me with a "serious misconception" of Dr. McTaggart's philosophy, as expounded in *The Nature of Existence*, in that I class McTaggart with Berkeley as a "spiritual pluralist," and attribute to McTaggart the view that the only genuine idealism is of the type of Berkeley's.

The point here raised by "L. S. S." is one which, I conceive, can be settled authoritatively by appeal to McTaggart's own words. Apart from many other passages, in *The Nature of Existence* and in other writings of McTaggart's, the passage which I chiefly had in mind when I classed McTaggart with Berkeley, is the following:—

"And thus, while the result which we shall reach will prove to be one which would be usually, and properly, called Idealism, it will be the idealism of Berkeley, of Leibniz, and—as I believe—of Hegel. It will not be the idealism of Kant, or of the school which is sometimes called neo-Hegelian. It will not, that is, be that idealism which rests on the asserted dependence of the object of knowledge upon the knowing subject, or upon the fact of knowledge, but the idealism which rests on the assertion that nothing exists but spirit" (*The Nature of Existence*, Bk. I., ch. iii., § 52, p. 50).

The term "spirit," in the formula "nothing exists but spirit," is further explained in a footnote attached to it as follows:—

"Spirit is here used to include, not only individual spirits, but the parts and groups of such spirits. And the characteristics of such spirits, and of their parts and groups, being characteristics of the existent, would themselves exist."

This passage and footnote, taken together, prove, I submit, unmistakably that McTaggart classed himself as an idealist of the same sort as Berkeley, i.e., as what I have called, following a usage established by others, a "spiritual pluralist".

It is true, no doubt, that McTaggart reaches this idealism by a road very different from Berkeley's. It is true, too, that his account of this plurality of spirits and of their relations is vastly more complex than that of Berkeley who, as I point out in my *Idealism* (pp. 97 ff.), has thought out the implications of his view only in the most superficial way.

But these two differences, though they would be important if one were trying to distinguish different types of spiritual pluralism, are irrelevant when the question simply is, Was McTaggart a spiritual pluralist at all? To this question he has given us his own answer, and that answer, it seems to me, does not support the criticisms which "L. S. S." brings against me. If McTaggart deliberately labels his own philosophy "idealism" in the same sense in which he applies that term to Berkeley's philosophy, it follows that the two philosophies have more in common than the name.

Moreover, I cannot follow "L. S. S."s suggestion that the affinities between Berkeley and Bertrand Russell are much closer than those between McTaggart and Berkeley. "L. S. S.," in taking this view, has in mind, presumably, Russell's theory of "sense-data" and "perspectives" in *The Problem of the External World*. But I can find there no sure indication that Russell conceives his plurality of perspectives as an actually existent plurality of spirits. Indeed, the probability seems to me to be the other way.

I may, perhaps, add that, obviously, I differ from McTaggart in the estimate of Hegel which he expresses in the passage quoted above, and elsewhere. Like the "neo-Hegelians," *i.e.*, presumably Caird, Bosanquet, and others, I class Hegel, not with Berkeley and Leibniz, but with Kant. Or, rather, I class Kant with Hegel as having initiated a new type of idealism which Hegel brought to its fullest development. And, again, I differ from McTaggart's view that in this idealism, the main emphasis lies on the "dependence of the object of knowledge upon the knowing subject". This is, certainly, a possible reading of Kant's *Critique of Pure Reason*, but in Hegel, so it seems to me, the emphasis has shifted in another direction. What I take this direction to be, I have tried to explain, so far as this was possible in a popular book intended for beginners, in my *Idealism*.

R. F. ALFRED HOERNLÉ.

PRINCIPIA MATHEMATICA.

TO THE EDITOR OF "MIND".

HARVARD UNIVERSITY,
5th Nov., 1925.

DEAR SIR,

I shall be glad if you will be good enough to insert the following statement:—

The great labour of supervising the second edition of the *Principia Mathematica* has been solely undertaken by Mr. Bertrand Russell. All the new matter in that edition is due to him, unless it shall be otherwise expressly stated. It is also convenient to take this opportunity of stating that the portions in the first edition—also reprinted in the second edition—which correspond to this new matter were due to Mr. Russell, my own share in those parts being confined to discussion and final concurrence. The only minor exception is in respect to *10, which preceded the corresponding articles. I had been under the impression that a general statement to this effect was to appear in the first volume of the second edition.

Truly Yours,

A. N. WHITEHEAD.

SIXTH INTERNATIONAL CONGRESS OF PHILOSOPHY.

[First Circular.]

In accordance with the Preliminary Announcement of August, 1924 the Sixth International Congress of Philosophy will meet, September, 1926, in the United States, under the auspices of the American Philosophical Association.

The date of the Congress will be 13-17th September, 1926.

The place will be Harvard University, Cambridge, Massachusetts, U.S.A. The President of Harvard and the Department of Philosophy extend to the Congress a hearty welcome. In particular, they are making convenient arrangements for the entertainment of Delegates and Members. Rooms will be provided for official Delegates free of charge. Meals for Delegates, rooms and meals for Members, will be furnished at minimum rates.

The recognised languages will be English, French, German, Italian, and Spanish.

The sessions of the Congress will be arranged under four divisions: A. Metaphysics (including the philosophy of nature, philosophy of mind and philosophy of religion); B. Theory of Knowledge, Logic, and Scientific Method; C. Theory of Values (including ethics, social philosophy, philosophy of the state, philosophy of law, philosophy of education and aesthetics); D. History of Philosophy. Under each of these divisions one General Session will be held with papers by specially invited speakers, whose names and subjects will be announced later. Sectional Meetings, in part on subjects determined in advance, will also be held under each division. The time allotted for sectional papers will be limited to twenty minutes each. Members offering papers are requested to forward, with the titles of their papers, brief typewritten summaries for submission to the Committee on the Programme. They are further requested to note that, to the regret of the Committee, it will be impossible to accept papers unless they are to be presented by their authors in person.

Membership in the Congress will include Active Members and Associate Members. Active Members will be entitled to all the privileges of the Congress, including a copy of the *Proceedings*. The fee for Active Members will be five dollars (\$5.00). The fee for Associate Members (families of Active Members and other qualified persons) will be two dollars and a half (\$2.50); but they will not be entitled to vote, to special privileges of entertainment, nor to the *Proceedings*.

Adhesions, Membership fees, and Papers for the Sectional Sessions may be sent to Professor John J. Coss, Corresponding Secretary, 531 West 116th Street, New York City.

Later circulars will contain the detailed plans for the Congress as they are developed and matured. Meanwhile, we commend it to the interest of our Colleagues in all lands; as, in behalf of the American Philosophical Association, we have the honour to invite them to join in continuing the series of International Congresses, inaugurated at Paris in 1900 for the advancement of philosophy and the promotion of intercourse among philosophical scholars.

(Signed)

For the Organizing Committee:

NICHOLAS MURRAY BUTLER, *Chairman*

A. C. ARMSTRONG, *Honorary Secretary.*

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